PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From November 21, 1878, *to April* 24, 1879.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

November 21, 1878.

Sir JOSEPH HOOKER, K.C.S.I., President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council proposed for election was read as follows:—

President.—William Spottiswoode, M.A., D.C.L., LL.D.

Treasurer.—John Evans, F.G.S., F.S.A.

Secretaries.— Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Thomas Henry Huxley, LL.D.

Foreign Secretary.—Alexander William Williamson, Ph.D.

Other Members of the Council.—Frederick A. Abel, C.B., V.P.C.S.; William Bowman, F.R.C.S.; William Carruthers, V.P.L.S.; Major-General Henry Clerk, R.A.; William Crookes, V.P.C.S.; Sir William Robert Grove, M.A.; Augustus G. Vernon Harcourt, F.C.S.; Sir Joseph Dalton Hooker, C.B., K.C.S.I., D.C.L.; Admiral Sir Astley Cooper Key, K.C.B.; Lieut.-General Sir Henry Lefroy, C.B.; Lord Lindsay, P.R.A.S.; Sir John Lubbock, Bart., V.P.L.S.; Lord Rayleigh, M.A.; Charles William Siemens, D.C.L.; John Simon, C.B., D.C.L.; Professor Allen Thomson, M.D., F.R.S.E.

The Presents received were laid on the table, and thanks ordered for them.

The Rev. Thomas George Bonney, Dr. John Hughlings Jackson, and Mr. Edward Alfred Schäfer were admitted into the Society.

General Boileau, General Clerk, Mr. J. Evans, Dr. Gladstone, and Mr. Simon having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

The following Papers were read:—

I. "On a method of using the Balance with great delicacy, and on its employment to determine the Mean Density of the Earth." By J. H. Poynting, B.A., Fellow of Trinity College, Cambridge, Demonstrator in the Physical Laboratory, Owens College, Manchester. Communicated by Professor B. Stewart, F.R.S. Received June 21, 1878.

[Plate 1.]

In the ease and certainty with which we can determine by the balance a relatively small difference between two large quantities, it probably excels all other scientific instruments.

By the use of agate knife edges and planes, even ordinary chemical balances have been brought to such perfection that they will indicate one-millionth part of the weight in either pan, while the best bullion balances are still more accurate. The greatest degree of accuracy which has yet been attained was probably in Professor Miller's weighings for the construction of the standard pound, and its comparison with the kilogramme, in which he found that the probable error of a single comparison of two kilogrammes, by Gauss's method, was $\frac{1}{140000000}$th part of a kilogramme.* ("Phil. Trans.," 1856.)

But, though the balance is peculiarly well fitted to detect the relatively small differences between large quantities, it has not hitherto been considered so well able to measure absolutely small quantities as the torsion balance. The latter, for instance, was used in the Cavendish experiment, when the force measured by Cavendish was the attraction of a large lead sphere upon a smaller sphere, weighing about $1\frac{1}{2}$ lbs., the force only amounting to $\frac{1}{50000000}$th part of this weight, or about $\frac{1}{5000}$th part of a grain.

The two great sources of error, which render the balance inferior to the torsion balance in the measurement of small forces, are:—

1. Greater disturbing effects produced by change of temperature, such as convection currents and an unequal expansion of the two arms of the balance.

2. The errors arising from the raising of the beam on the supporting frame between each weighing, consisting of varying flexure of the beam and inconstancy of the points of contact of the knife edges and planes.

The disturbances due to convection currents interfere with the torsion balance as well as with the ordinary balance, though they are

* Even so far back as 1787, Count Rumford used a balance which would indicate one in a million and measure one in seven hundred thousand. ("Phil. Trans.," 1799.)

more easily guarded against with the former, by reason of the nature of the experiments usually performed with it. They might, perhaps, as has been suggested by Mr. Crookes, be removed from both by using the instruments in a partial vacuum, in which the pressure is lowered to the "neutral point," where the convection currents cease, but the radiometer effects have not yet begun. But a vacuum balance requires such complicated apparatus to work it, that it is perhaps better to follow the course which Baily adopted in the Cavendish experiment. He sought to remove the disturbing forces as much as possible, and to render those remaining as nearly uniform as possible in their action during a series of experiments, so that they might be detected and eliminated. For this purpose the instrument was placed in a darkened draughtless room, and was protected by a thick wooden casing gilded on its outer surface. Most of the heat radiated from the surrounding bodies was reflected from the surface of the case by the gilding. The heat absorbed only slowly penetrated to the interior, and was so gradual in its action, that, for a considerable time, the effect might be supposed nearly uniform. Under this supposition it was then eliminated by the following method of taking the observations. The resting point (that is the central position of equilibrium, about which the oscillations were taking place) of the torsion rod, at the ends of which were the small attracted weights, was first observed when the two large masses pulled it in one direction. The masses were then moved round to the opposite side, when they pulled the rod in the opposite direction and the resting point was again observed. The masses were then replaced in their original position and the resting point was observed a third time. These three observations were made at equal intervals of time; if, then, the disturbing effect was uniform during the time, the mean of the first and third observations gave what the resting point would have been, had the rod been pulled in that one direction at the same time that it was actually observed when pulled in the opposite direction. The difference between the second resting point and the means of the first and third might, therefore, be considered as due to the attractions of the masses alone.

In the experiments of which this paper contains an account, I have endeavoured to apply this method of introducing time as an element to the ordinary balance. But, before it could be properly applied, it was necessary to remove the errors due to the raising of the beam between successive weighings, as they could not be considered to vary in any uniform way with the time. I think I have effected this satisfactorily, by doing away altogether with the raising of the beam by the supporting frame, between the weighings. For this purpose I have introduced a clamp underneath one of the pans, which the observer can bring into action at any time, to fix that pan in whatever position it may be. The weight can then be removed from the pan,

and another, which is to be compared with it, can be inserted in its place without altering the relative positions of the planes and knife edges. The counterpoise in the other pan, meanwhile, keeps the beam in the same state of flexure. The pan is then unclamped and the new position about which it oscillates is observed. The only changes are due to the change in the weight and the effect of the external disturbing forces; the latter we may consider as proportional to the time, if sufficient precautions have been taken, and by again changing the weights and again observing the position of the balance, we may eliminate their effects.

Though the method when applied to the balance does not yet give such good results as Baily obtained from the torsion balance—partly, I believe, because I have not yet been able to apply all his precautions to remove external disturbing forces—it still gives better results than would have been obtained without it. This may be seen by the numbers recorded in the tables, where a progressive motion of the resting point may be noticed in most cases, in the same direction, during a series of experiments. Even when this is not the case, the method at once shows when the disturbing forces are irregular, and when we are justified in rejecting an observation on that account.

I give in this paper two applications of the method, one to the comparison of two weights, the other to the determination of the mean density of the earth. The latter is given only as an example of the method, but I hope shortly to continue the experiments with a large bullion balance, for the construction of which I have had the honour to obtain a grant from the Society. The balance is now in course of construction, by Mr. Oertling, of London.

Description of the Apparatus.

The balance which I have employed is one of Oertling's chemical balances, with a beam of nearly 16 inches, and fitted with agate planes and knife edges. It will weigh up to a little more than 1 lb. To protect it from sudden changes of temperature, the glass panes of the case are covered with flannel, on both sides of which is pasted gilt paper, with the metallic surface outwards. This case is enclosed in another outer case, a large box of inch deal, lined inside and out with gilt paper. The experiments have been conducted in a darkened cellar under the chemical laboratory at Owens College, which was kindly placed at my disposal by Professor Roscoe. As the ceilings and floors of the building are of concrete, any movement near the room causes a considerable vibration of the floor and walls. It was necessary, therefore, to support the balance independently of the floor. For this purpose, six wooden posts (A, B, C, D, E, F, fig. 1) were erected resting on the ground underneath and passing freely through the floor to a height of 6 feet 6 inches above it. They are connected at

the top by a frame like that of the table, and stayed against each other to give firmness. The wider part of the frame, near the posts E and F, is boarded over to form a table for the telescope (*t*, fig. 1) and scale (*s*), by which the oscillations of the balance are observed. The box containing the balance rests on two cross pieces, on the narrower part, ABCD, of the frame, with the beam parallel to AD, and its right end towards the telescope.

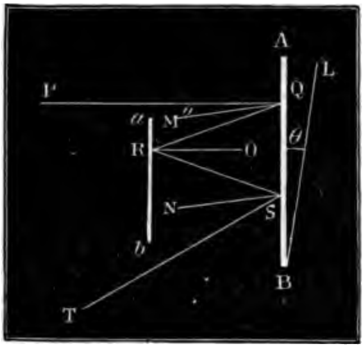
In order to observe the position of the beam, a mirror, $1\frac{1}{4}$ inches by $\frac{3}{4}$ inch, is fixed in the centre of the beam, and the reflection of a vertical scale (*s*, fig. 1) in this is viewed with a telescope (*t*) placed close to the scale. The light from the scale passes through two small windows cut in each of the cases of the balance and glazed with plate glass. The position of the beam is given by the division of the scale upon the cross line on the eyepiece of the telescope. The scale, which was photographed on glass, and reduced from a large scale, drawn very carefully, has 50 divisions to the inch. These are ruled diagonally with ten vertical cross lines. It is possible to read, with almost certainty, to a tenth of a division, or $\frac{1}{500}$th of an inch. Since the mirror is about 6 feet from the scale, a tenth of a division means an angular deflection of the beam of about 3″.*

The scale is illuminated from behind by a mirror (*m*), several inches in diameter, which reflects through it a parallel beam from a paraffin lamp (*l*). A plate of ground glass between the scale and mirror diffuses the light evenly over the scale and, by altering the position of the mirror, any desired degree of brilliancy may be given to the illumination of the scale. A screen (not shown in fig. 1) prevents stray light from striking the balance-case.

This method of reading—which, of course, doubles the deflection—has been so far sufficiently accurate for my purpose; that is to say, the errors arising from other sources are far greater than those arising from imperfections of reading. But in a long series of preliminary experiments I used the following plan to multiply the deflection still further. A rather smaller fixed mirror, *ab*, is placed opposite to and facing the beam-mirror, AB, fixed on the beam, and a few inches from it. Suppose the beam-mirror to be deflected from the position BL, parallel to *ab*, through an angle, θ, to the position AB. If a ray, PQ, perpendicular to *ab* strikes AB at Q, it will make an angle, θ, with QM, the normal at Q, and will be reflected along QR, making an angle, 2θ, with its original direction, and therefore with the normal RO, at R, when it strikes it. If it be reflected again to AB at S, it will make an angle, 3θ, with the normal SN, and the reflected ray, ST, will make an angle, 4θ, with the original direction, PQ, of the ray. It may be still further reflected between the two mirrors, if

* The numbers on the scale run from below upwards, so that an increase in the weight in the right hand pan is indicated by a lower number on the scale.

desirable, each reflection at the mirror, AB, adding 2θ to the deflection of the ray. I have, for instance, employed three reflections from the



beam-mirror, so multiplying the deflection six times. In this case, one division of my scale, at the distance at which it was placed from the beam, corresponded to a deflection of 7″ in the beam, and this could be subdivided to tenths by the eye. The only limit to the multiplication arises from the imperfection of the mirrors and the decrease in the illumination of the successive reflections.*

The chair of the observer is placed on a raised platform, and a small table rising from the platform and free from the frame on which the instruments rest, is between the observer and the telescope. On this he can rest his note-book during an experiment. As the differences of weight observed are sometimes exceedingly minute, the balance is made very sensitive—usually vibrating in periods between 30″ and 50″. The value of a division of the scale cannot be determined by adding known small weights to one pan, as the deflection would usually be too great. Any approach of the observer to the case causes great disturbances, so that the ordinary method of moving a rider an observed distance along the beam is inapplicable. In some experiments made last year I calculated the force equivalent to the small differences in weight, in absolute measure, by observing the actual angular deflection and the time of vibration. With a knowledge of the moment of inertia of the beam and treating it as a case of small oscillations, it was possible to calculate the value of the scale. But the observations and subsequent calculations were so complicated that the following method of employing riders was ultimately adopted.

A small bridge about an inch long (fig. II, 1) is fitted on to the beam. The sides of the bridge are prolonged about half an inch above the

* This method was used in the seventh and eighth series here recorded. Two reflections from the beam mirror were employed, giving four times the actual deflection.

arch which fits on to the beam, as shown in the end view (fig. II, 2). In each of these sides are cut two V-shaped notches directly opposite to each other, one of the opposite pairs being 6·654 millims. (about ¼ inch) distant from the other pair. Two equal riders of the shape shown in fig. II, 3, are placed across the bridge, and are of such a size that they will just fit into the bottom of the notches. When one of these rests across the bridge the other is raised up from it. The lowering of one rider and the raising of the other corresponds herefore to a transference of a single rider from one pair of notches to the other. The length of the half beam being 202·716 millims. and the distance between the notches 6·654 millims., this transference will be equivalent to the addition to one pair of 3·03284 of the weight of the rider used. As I have generally used a centigramme rider this means 0·3284 mgms.

Two levers *ll'* (fig. II, 4), with hooks *hh'* are used to raise one rider while the other is lowered. These levers are worked by two cams *cc'* on a rod R, which is prolonged out of the balance case to the observer. By turning this rod round, the one lever is raised while the other is depressed. The hook at the end of the raised lever picks up its rider while the other hook deposits its rider on the bridge, and then sinks down between the raised sides (as shown in fig. II, 4), leaving the rider resting freely on the bridge.

The levers are so adjusted that the beam even in its greatest oscillations never comes in contact with the hooks.

This arrangement might probably be still further perfected by introducing two small frames for the riders to rest upon, the frames resting on the beam by knife edges. It would then be certain that the movement of the riders was equivalent to a transference from one knife edge to the other, whereas the rider at present may not rest exactly over the centre of the notch. But I find that I get fairly consistent results by lowering the rider somewhat suddenly so as to give it sufficient impetus to go to the bottom of the notch, and have not therefore thought it necessary as yet to introduce more complicated apparatus.

In place of the right hand pan of the usual shape, another of the shape shown in fig. III, 1, is employed. To the centre of the pan underneath is attached a vertical brass rod which passes downwards through the bottom of the inner case of the balance. To the under side of this case is attached the clamping arrangement before referred to. This consists of two sliding pieces (fig. IV, 1, *ss*) working horizontally in a slot cut in a thick brass plate which is fastened to the case. Through a circular aperture in this plate (the slot is not cut through the whole thickness of the plate, but only as shown in fig. IV, 2) and about the middle of the slot hangs the rod *r* attached to the scale pan.

By means of right and left handed screw on a rod R, which is pro-

longed out of the case to the observer, these two sliding pieces can be made to approach, and clamp the rod, or to recede and free it. By having the opposite surfaces of the sliding pieces and the rod polished and clean, it is possible to clamp and unclamp without producing any disturbance. The clamp is of great use also to lessen the vibrations when they are too large, as it may be brought into action at any moment, and on releasing carefully the beam will start again from rest without any impetus. It may be used too to increase the vibrations by releasing suddenly when the beam will have a slight impetus in one direction or the other.

The weights which I have compared are two brass pounds avoirdupois, made for me by Mr. Oertling, and marked A and B respectively. They are of the usual cylindrical shape with a knob at the top (fig. III, 2). Two small brass pans (fig. III, 3) with a wire arch by which they can be suspended, are used to carry them; these are called respectively X and Y. I found on beginning to use them that there was too great a difference between A and B. I therefore adjusted them by putting a very small piece of wax upon A, the lighter. But the difference between them increased by 0·0782 mgm. in two days, which I thought was probably due to the wax. After the fourth series I therefore removed it and scraped B till it was more nearly equal to A. The weighings I—IV have, however, been retained, for though the differences on different days vary they are fairly constant on the same day.

The weights are changed by the following apparatus which has been designed to effect the change as simply and quickly as possible.

A horizontal "side rod" or link (*ss*, fig. V) is worked by two cranks (*cc*, fig. V, 2), which are attached to the axles of two equal toothed wheels (*tt*) with a pinion (*p*) connecting them. A second pinion (*q*), on a rod prolonged out of the case to the observer, gears with one of the toothed wheels. By turning this rod the toothed wheels are set in motion, both in the same direction, moving the horizontal "side rod" from the right say upwards and over to the left. A pin (*pn*) stops its motion downwards further than is shown in fig. V, 1. Near each end of the rod is cut a notch, and across these are hung the pans carrying the weights. The apparatus is fastened to the floor of the case between the central upright supporting the beam and the scale pan, the side rod being perpendicular to the direction of the beam, and exactly over the centre of the pan. In fig. V, 1, one of the weights B is supposed to be resting on the scale pan (the wires suspending the pan from the beam not being shown), the side rod having moved down so far below the wire of the smaller pan carrying the weight that it leaves it quite free. If, now, it is desired to change the weights the rod R is turned, setting the wheels in motion, the side rod moves up, picks up B—the notch catching the wire—then travels

over round to the extreme right, when A will be just over and nearly touching the scale pan. By continuing the motion slightly A will be gently deposited on the pan, and the side rod will move slightly down leaving the weight quite free. On the scale pan are four pins, turned slightly outwards, acting as guides for the small pan, and ensuring that it shall always come into the same position. The wheels and pinions are of such a size that two revolutions of the rod just suffice to change one weight for the other.

It will be seen that all the manipulation required from the observer during a series of weighings is the simple turning of three rods, which are prolonged out of the balance case to where he is stationed at the telescope. By turning one of these he can change the position of the rider on the beam by a known amount, and so find the value of his scale. By turning a second he clamps the scale pan, and so steadies the balance while the weights are changed by turning a third rod. I have made this arrangement not only because it seems as simple as possible to secure the end required, but also because it seemed more applicable to a vacuum balance (with which I hope ultimately to test it).

I take this opportunity of expressing my thanks to Mr. Thomas Foster, mechanician of Owens College, for his aid in the construction of the apparatus, and in the planning of many of its details.

Method of conducting a Series of Weighings.

After the counterpoise has been adjusted so that the beam swings nearly about its horizontal position, the frame is lowered so that the balance is ready for use. The pan is then clamped and the balance is left to come to a nearly permanent state of flexure if possible, sometimes for the night or even longer. The lamp is lighted usually half-an-hour or more before beginning to observe, so that its effect on the balance may attain a more or less steady state. It is necessary also to wait some time after coming into the room, for the opening of the door will always cause a considerable and immediate deflection of the beam. When a sufficient time has elapsed, the observations are commenced with a determination of the value of one scale division by means of the riders. The three extremities of two successive oscillations are observed with one of the riders resting on the beam. These are then combined as follows:—The mean of the first and third is taken, and the mean again of this and the second, this constituting the "resting point," that is, the position of equilibrium of the beam at the middle of the time. For instance, in weighing No. I (see tables at the end) the three extremities of successive oscillations were 280·5, 312·0, and 286·0 (column 2). The resting point was taken as—

$$\frac{280{\cdot}5+286{\cdot}0+2\times312{\cdot}0}{4}=297{\cdot}62,$$

the rider on the beam being the right hand one denoted by R, column 1. The balance is the clamped, and the other rider is brought on to the beam while the first is taken up. The resting point is again observed. In No. I it was 270·05. The balance is again clamped, and the first rider again brought on the beam, and in unclamping the resting point again observed. In the same weighing it was 296·75. These three are sufficient to give one determination of the deflection due to the transference of a rider. This will be the difference between the second resting point and the mean of the first and third. For instance, $\frac{297{\cdot}62+296{\cdot}75}{2}-297{\cdot}18=27{\cdot}13$ divisions. This number is found in the fifth column.

This process is continued, the resting points being combined in threes till several values of the deflection due to the rider have been obtained, and the mean of these is taken as the true value. This plan of combining the resting points requires that the observations should be taken at nearly equal intervals. After a little practice it will always take the observer about the same time to go through the same operations of clamping, changing the riders, unclamping, clamping again to lessen the vibrations about the new resting point, and then beginning to observe, and I have considered that this was a sufficiently correct method of timing the observations.

When a series has been taken it will at once be seen whether they were begun too soon after entering the room, or whether any irregular disturbing force has acted. For instance, in weighing No. II, determination of one scale division, the first resting point is so much lower than the succeeding with the same rider that evidently the balance was still affected by my entrance into the room. It was, therefore, rejected. Again, in weighing No. III determination of the difference between the weights, the fourth resting point was much lower than the others with the same weight in the pan. The resting points, when the other weight was in the pan, showed no similar sudden drop of such magnitude. This observation was, therefore, rejected as being affected by some irregular disturbance.

When the value of the deflection is determined, the value of one scale division is at once found by dividing ·3282 mgm. by the number of divisions of the deflection, since the charge of the sides is equivalent to the addition of ·3282 mgm. to one pan.

The determination of the difference between the weights is then begun. This is carried on in a precisely similar manner, the only difference being, that the rod changing the weights is now turned round in place of the rod changing the riders. I have usually taken a greater number of observations of the difference between the weights than of the deflection due to the riders, as the former is somewhat more irregular than the latter. This irregularity I believe to arise

from slight differences of temperature of the two weights, and perhaps from air currents caused by their motion inside the case. They do not seem to be due to any fault in the clamping arrangement, since that is employed equally in both, and the changing of the weights, if effected gently, does not move the beam at all.

When the deflection has been determined, it is multiplied by the number of milligrammes corresponding to one scale division, and this, of course, gives the difference between the weights. I have interchanged the weights in the two pans X and Y, between the series of weighings, in order to make the experiments like those conducted in the weighings for the standard pound. But my object has not been to show at all that the method gives consistent results day after day, and, in fact, the difference between the weights has varied. For instance, according to weighings I and II, $A-B=\cdot 0446$, while, according to weighings III and IV, $A-B=\cdot 0232$. There is a greater difference between these than can be accounted for by errors of experiment, and it probably arose from the small piece of wax with which I made A nearly equal to B. The difference between the weights when measured to such a degree of accuracy as that which I have attempted, will, no doubt, vary from time to time, partly with deposits of dust, partly with changes in the moisture in the atmosphere, and so on.

But I think the numbers which are given in the tables are sufficient to show that the difference between two weights in any one series of weighings can be measured with a greater degree of accuracy than has hitherto been supposed possible. I give in the tables a full account of the weighings, each series containing a determination of the value of one scale division and a determination of the difference between the weights. The greatest deviation of any one of a series from the mean of that series of differences is always given. This I consider a better test of accuracy of weighing than the probable error. What is wanted in weighing is rather a method which will give at once a good determination of the difference between two weights. But I may state, that if the error of any one of a series be taken as its difference from the mean of that series, the probable error of a single determination of the difference between the weights in the first four series is ·4344 of a division, or ·0054 mgm., that is, $\frac{1}{84000000}$th of the total weight, while the greatest error is 1·8 divisions, or ·0224 mgm., that is $\frac{1}{20000000}$th of the total weight. It may be remarked that these weighings were all made during peculiarly unfavourable weather when there were frequent heavy showers, causing sudden changes of temperature, and thus seriously affecting the working of the balance. In the series V—VIII the greatest error is only $\frac{1}{80000000}$ of the total weight, the weather having improved considerably.

On the Employment of the Balance to determine the Mean Density of the Earth.

In the Cavendish experiment, the attraction of a large sphere of lead of known mass and dimensions upon another smaller sphere also of known mass and dimensions, is measured when the two are an observed distance apart. Comparing this attraction with the weight of the small sphere—that is the attraction of the earth upon it—and knowing the dimensions of the earth, we can deduce the mass of the earth in terms of the mass of the large lead sphere, and so obtain its mean density. The torsion balance, which was invented for the purpose by Mitchell, the original contriver of the experiment, has hitherto been used to determine the force exerted by the mass upon the small sphere. In the arrangement here described, I have replaced the torsion balance by the ordinary balance, and have so been able to compare the attraction of a lead sphere with that of the earth upon the same mass somewhat more directly. The results which I have obtained have no value in themselves, but they serve as an example of the employment of the balance for more delicate work than any which it has as yet been supposed able to perform.

The method is shortly this :—A lead weight (called "the weight") weighing 452·92 grms. (nearly 1 lb.) hangs down by a fine wire from one arm of a balance, from which the pan has been removed at a distance of about six feet below it, and is accurately counterpoised in the other pan, suspended from the other arm. A large lead mass (called "the mass") weighing 154,220·6 grms. (340 lbs.) is then introduced directly under the hanging weight. The attraction of this mass increases the weight slightly and the beam is deflected through an angle which is observed. The value of this deflection in milligrammes is measured by the employment of riders in the manner described above, and so the attraction of the mass is known. The increase of the weight caused by the mass has been in my experiments about ·01 of a milligramme, or $\frac{1}{450000000}$th of the whole weight.

The balance which I have used is that which I have described above. It was placed in the same room and in the same position as in the weighing experiments. The same method was used to observe the oscillations with a single mirror on the beam. The scale was a simple one etched on glass and not diagonally ruled. It had about 50 divisions to the inch, and the numbers increased from above downwards, so that an increase in the weight hanging from the left arm was indicated by a lower number on the scale.

The weight which is suspended by a very fine brass wire from the left arm, passing through a hole in the bottom of the balance case, hangs in a double tin tube, 4 inches in diameter, to protect it from air currents. At the bottom of the tube is a window, through which can

be seen the bottom of the weight as it hangs. The weight is 4·248 centims. in diameter and is gilded. The mass is a sphere of an alloy of lead and antimony. It was cast with a "head" on and then accurately turned. Its vertical diameter is 30·477 centims. (about 1 foot). The specific gravity of a specimen of the metal was found to be 10·422. Its weight given by a weighing machine is 340 lbs. about, and this agrees very nearly with the weight calculated from the specific gravity. I am obliged to accept this as the true weight provisionally, until it is found more correctly by the large balance referred to above and now being constructed.

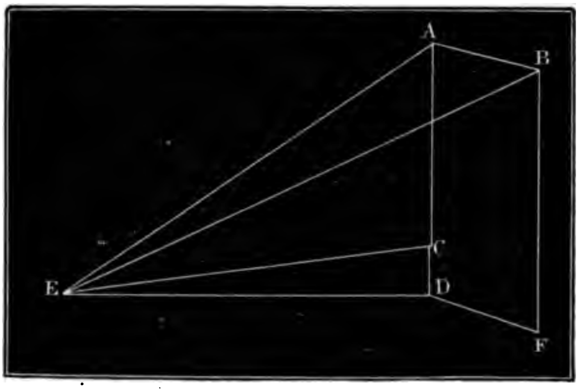
This mass (fig. I, M) is placed in a shallow wood cup at one end of a 2-inch plank, 8 inches wide and 6 feet 11 inches long, mounted on four flanged brass wheels, and serving as a carriage for it (fig. I). A plank about 12 feet long nailed to the floor in a direction perpendicular to the beam of the balance, as shown in fig. I, *pp*, acts as a railway for the carriage, and a firm stop at each end prevents the carriage from running off the rail. The distance between the stops is rather less than twice the length of the carriage, and the weights hangs down from the balance exactly midway between the stops. The mass is placed on the carriage so that it is exactly under the weight when the carriage is at one end of its excursion against one of the stops. An empty cup (*c*, fig. 1) of the same dimensions as that in which the mass rests is placed at the other end of the carriage, and is just under the weight when the carriage is against the other stop. By this arrangement no correction is needed for the attraction of the carriage upon the weight or counterpoise, and the effect caused by the removal of the carriage from one end of its excursion to the other is entirely due to the difference of attractions of the mass upon the weight and counterpoise in its two positions. The position of the mass when directly under the weight is called its "in position," and that when it is at the other end of its excursion is called the "out position." The length of the excursion is 5 feet 7·3 inches.

To draw the carriage along the rail a vertical iron shaft with a wood cylinder at the lower end pivots on the floor, and is prolonged up to the level of the observer as he sits at the telescope with a handle by which he can turn it. The two ends of a rope which winds round the cylinder pass through pulleys on the stops, and are attached to the ends of the carriage. The observer can then move the mass with great ease by turning the handle, even while looking through the telescope.

When a series of observations is made, the general method is this. The deflection (r) due to the transference of a rider from one notch to the other on the beam is first observed exactly in the manner before described, the mean of four or five values being taken as the true value. Then the deflection (n) due to the difference of attraction of

the mass in its two positions is found in exactly the manner in which the difference between two weights is found, except that now when three successive extremities of oscillations have been observed for a resting point the mass is moved from one position to the other where the weights were changed in the former experiments, the clamp not being brought into action. The second extremity of the oscillation which is proceeding while the mass is moved, is observed as the first of the next three. When nine or more resting points have been observed they are combined in threes, and the mean of the resulting values of the deflection n is used in the subsequent calculation.

This deflection is, of course, less than that which would be observed were there no attraction on the counterpoise, and were the out position of the mass at an infinite distance. To find the factor f by which the deflection n due to the change of position of the mass must be multiplied in order to reduce it to the deflection which would be observed under these conditions, let AB be equal and parallel to the beam



of the balance at the level of the counterpoise of which B is the centre. Let C be the centre of the weight, D that of the mass in its in position, E that of the mass in its out position. Draw BF, DF, parallel to AD, AB. Let $\mu =$ the mass.

The vertical attraction of the mass in its in position will be—

$$\frac{\mu}{CD^2} - \frac{\mu BF}{BD^3}.$$

The vertical attraction in its out position will be—

$$\frac{\mu CD}{CE^3} - \frac{\mu BF}{BE^3}.$$

The difference between these is actually observed, viz.:

$$\frac{\mu}{CD^2}\left\{1 - \frac{CD^2 BF}{BD^3} - \frac{CD^3}{CE^3} + \frac{CD^2 BF}{BE^3}\right\}.$$

The factor by which we must multiply the observed difference to reduce it to the attraction of the mass on the weight in its in position is therefore—

$$f=1+\frac{CD^2BF}{BD^3}+\frac{CD^3}{CE^3}-\frac{CD^2BF}{BE^3}$$
$$=\ 1\cdot 0185$$

since CD= 22·13 centimetres.
BD=192·03 ,,
BF=187·70 ,,
CE=172·36 ,,
BE=257·09 ,,

The values of r and n being observed, the distance between the centres of the mass and weight d is then measured by adding 17·362 centims. to the sum of their radii, the distance from the top of the mass to the bottom of the weight as measured by a cathetometer.

It now remains to explain the calculation of the mean density Δ from the observed values of r, n, and d. We have—

$$\frac{f\times\text{increase in weight observed}}{\text{Weight of weight}}=\frac{\text{Attraction of mass on weight when ``in''}}{\text{Attraction of earth on weight}}.$$

But the increase in weight is $\frac{n\times 6654}{r\times 202716}$ mgms., since the distance between the notches is 6·654 millims., and the half beam 202·716 millims. The weight of the weight is 453·92 grammes.

The attraction of the mass in centimetres

$$=\frac{\text{Volume}\times\text{density}}{(\text{distance between centres of mass and weight } r)^2},$$
$$=\frac{\text{Weight in grammes}}{d^2},$$
$$=\frac{154220\cdot 6}{d^2}.$$

The attraction of the earth is

$$\Delta\times\frac{4}{3}\pi R\left\{1+M-\frac{5}{2}(M-\epsilon)\right\}\cos^2\lambda,$$

where Δ=mean density of the earth,
R=earth's polar radius in centimetres,
$M=\frac{\text{centrifugal force at the equator}}{\text{Equatorial gravity}}$,
ϵ=ellipticity.
λ=latitude.

The logarithm of the coefficient of Δ when R is in inches is 9·0209985 ("Astronomical Soc. Mem.," xiv, p. 118), or if R is in centimetres it is 9·4258322.

Inserting these values in equation A we obtain

$$\Delta=\frac{\dfrac{154220\cdot 6}{d^2}}{\dfrac{4}{3}\pi R\left\{1+M-\left(\dfrac{5}{2}M-\epsilon\right)\cos^2\right\}\lambda}\times\frac{453290}{f\times\dfrac{n}{r}\dfrac{6654}{202716}}.$$

$$\therefore \Delta=C\times\frac{r}{nd^2}$$

$$\text{where } C=\frac{15220\cdot 6\times 453290\times 202716}{\dfrac{4}{3}\pi R\left(1+M-\dfrac{5}{2}\overline{M-\epsilon}\right)\cos^2\lambda\times f\times 6654}$$

and $\log C=1\cdot 8951337$,

$\therefore \log \Delta=1\cdot 8951337$,
$+\log r$,
$-\log n$,
$-2\log d$.

The following table is an account of an experiment made on May 30th, 1878, and will serve as a specimen of the method of making the observations. It is the best which I have yet made in the closeness with which all the values of n agree with each other.

VII.—May 30, 1878.—Determination of r.

Rider on beam.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Of r differences $=r$.
R	260·9 250·9 260·6	255·82		
L	214·6 205·0 212·3	209·22	256·74	47·52
R	271·9 246·7 269·4	257·07	210·31	47·36
L	214·3 209·2 212·9	211·40	257·62	46·22
R	249·7 263·8 253·0	257·57	212·23	45·34
L	204·9 220·7 206·0	213·07		

Mean $r=46\cdot 61$.

Determination of n.

Position of mass.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences $=n$.
In	216·8 208·1 215·9	212·22		
Out	210·9 216·1 211·0	213·52	212·27	1·25
In	213·9 210·8 213·8	212·32	213·61	1·29
Out	212·7 214·6 212·9	213·70	212·41	1·29
In	211·5 213·6 211·3	212·50	213·78	1·28
Out	215·9 211·8 216·0	213·87	212·60	1·27
In	210·2 215·0 210·6	212·70	213·93	1·23
Out	216·7 211·6 216·1	214·00	212·73	1·27
In	209·9 218·4 210·4	212·77	214·08	1·31
Out	217·0 211·4 216·9	214·17	212·98	1·19
In	209·8 216·2 210·6	213·20		

$\therefore$ mean $n = 1{\cdot}26$.

At the close of the experiment d was found to be 22·216 centimetres. We have therefore—

$$\begin{aligned}\log \Delta &= \log C.\\ &\quad +\log 46{\cdot}61.\\ &\quad -\log 1{\cdot}26.\\ &\quad -2\log 22{\cdot}16.\\ &= 1{\cdot}8951337.\\ &\quad +1{\cdot}6684791.\\ &\quad -0{\cdot}1003705.\\ &\quad -2{\cdot}6937226.\\ &= {\cdot}7695197.\\ \therefore \Delta &= 5{\cdot}882.\end{aligned}$$

I have made in all 11 experiments with this method. The resulting values of Δ are—

1	May 20	5·393.
2	„ 23	5·570.
3	„ 24	4·415.
4	„ 28	7·172.
5	„ 29	5·109.
6	„ 29	6·075.
7	„ 30	5·882.
8	„ 30	6·336.
9	June 5	5·977.
10	„ 5	5·580.
11	„ 6	5·100.

The resulting mean value of the mean density of the earth is 5·69.

If the eleven determinations be supposed to have equal weight, the probable error of their value is 0·15.

The various determinations differ very much among themselves, but they seem to me sufficiently close to justify the hope that with a large balance and a large weight, which will not be so easily affected by air currents, and with greater precautions to prevent those air currents, a good determination of the mean density of the earth may ultimately be obtained by this method.

I.—June 12.—Determination of Value of 1 Scale Division.

Rider on the beam.	Extremities of three successive oscillations.	Resting point.	Means of preceding and successive resting points.	Differences, *i.e.*, deflection due to ·3282 mgm.
L	280·5 312·0 286·0	297·62		
R	253·1 284·9 257·3	270·05	297·18	27·13
L	306·2 288·5 303·8	296·75	268·68	28·07
R	258·8 275·0 260·5	267·32	294·81	27·49
L	285·8 298·7 288·3	292·87	266·79	26·08
R	272·9 260·6 271·0	266·27	292·88	26·61
L	296·1 290·0 295·5	292·90		

Mean R—L=27·08 divisions.

$$\therefore 1 \text{ division} = \frac{0·3282}{27·08} = 0·01212 \text{ milligramme.}$$

Determination of (B+X)−(A+Y).

Weight on pan.	Extremities of oscillations.	Resting point.	Means of preceding and succeeding resting points.	Differences.
A+Y	254·5 272·9 257·3	264·40		
B+X	267·5 249·8 264·8	257·97	263·27	5·30
A+Y	258·5 265·4 259·3	262·15	257·04	5·11
B+X	259·3 253·6 258·0	256·12	261·87	5·75
A+Y	253·5 268·5 255·9	261·60	255·63	5·97
B+X	244·6 264·5 247·0	255·15	261·87	6·72
A+Y	273·6 252·0 271·0	262·15	255·85	6·30
B+X	252 7 260 253·5	256·55	262·90	6·37
A+Y	275 253·7 272·4	263·70	..	Mean difference 5·93

$$\therefore (B+X)-(A+Y) = \cdot 01212 \times 5\cdot 93.$$
$$= \cdot 0718 \text{ milligramme.}$$

Greatest deviation from the mean—

$$= \cdot 82 \text{ division.}$$
$$= \cdot 0099 \text{ milligramme.}$$

The weather during this series of weighings was very unfavourable, with frequent heavy showers.

II.—June 13.—Determination of 1 Scale Division.

Rider on beam.	Extremities of oscillations.	Resting point.	Means of preceding and succeeding resting points.	Differences due to R−L.
L	302·8 299·9 302·9	301·37	This is rejected as it is so much lower than the succeeding.	
R	321·8 335·7 326·8	330·00	..	
L	299·6 308·7 301·0	304·50	330·68	26·18
R	322·9 337·5 327·6	331·37	305·10	26·27
L	299·5 310·9 301·5	305·70	331·77	26·07
R	325·2 337·1 329·3	332·17	305·90	26·27
L	290·4 319·4 295·2	306·10	..	

∴ mean R−L=26·20.

∴ 1 division=·01252 milligramme.

The weather was as unfavourable as on the previous day.

The weights were changed shortly before the commencement of this series and the balance then worked so irregularly that for some

time I was unable to begin the rider determination. Even then the first resting point had to be rejected.

Determination of (A+X)−(B+Y).

Weight in pan.	Extremities of oscillations.	Resting point.	Means of preceding and succeeding resting points.	Differences.
B+Y	307·8 326·0 311·6	317·85	These are all rejected, as the motion was so irregular. The weights had been changed a short time before, and had probably not reached an uniform temperature.	
A+X	293·7 307·8 295·8	301·27		
B+Y	309·6 322·3 312·6	316·70		
A+X	295·5 309·6 297·6	303·17		
B+Y	304·1 322·7 308·1	314·4		
A+X	289·3 304·4 291·5	297·4		
B+Y	305·5 315·0 307·5	310·75	297·35	13·40
A+X	294·0 300·1 294·8	297·30	309·95	12·65
B+Y	304·6 312·7 306·6	309·15	296·51	12·54
A+X	290·0 300·7 291·5	295·72		

Mean (A+X)−(B+Y)=12·86 divisions.

∴ (A+X)−(B+Y)=0·1610 milligramme.

Greatest deviation from mean =0·54 division=0·0067 milligramme.

III.—June 13.—Determination of 1 Scale Division.

Rider on beam.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences due to R–L.
R	301·3 290·6 299·7	295·55		
L	313·7 328·7 318·1	322·30	296·38	25·92
R	293·6 300·4 294·5	297·22	322·47	25·25
L	311·0 331·6 316·4	322·65	297·88	24·77
R	281·6 313·2 286·2	298·55	323·57	25·02
L	310·1 335·8 316·3	324·50	298·62	25·88
R	287·8 308·1 290·8	298·7		

Mean R–L=25·37 divisions.

$$\therefore 1 \text{ division} = \frac{\cdot 3282}{25\cdot 37} = \cdot 01293.$$

Determination of (A+X)−(B+Y).

Weight in pan.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences.
A+X	303·3 294·5 301·8	298·52		
B+Y	303·6 315·8 306·2	310·35	297·07	13·28
A+X	288·9 301·5 290·6	295·62		
B+Y	300·2 311·6 302·5	306·47	This is evidently due to some irregular and short disturbing cause, and is rejected.	
A+X	288·8 304·1 291·1	297·07		
B+Y	301·5 319·3 303·9	311·0	297·16	13·84
A+X	291·7 302·0 293·2	297·25	310·33	13·08
B+Y	301·0 316·8 304·1	309·67		

Mean $(A+X)-(B+Y)=13{\cdot}40$ divisions.

$\therefore (A+X)-(B+Y)=0{\cdot}1732$ milligramme.

Greatest deviation from mean $=\cdot44$ division $=\cdot0057$ milligramme.

IV.—June 14.—Determination of 1 Value of Scale Division.

Rider on beam.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences due to R−L.
R	252·5 253·4 251·7	252·5	This was taken soon after entering the room. It is so much lower than the succeeding that it is rejected.	
L	290·8 273·7 288·8	281·75		
R	242·4 266·7 245·1	255·22	281·96	26·74
L	272·6 289·7 276·7	282·17	255·89	26·28
R	252·8 253·9 258·7	256·57	282·67	26·10
L	286·2 280·0 286·5	283·17		

Mean R−L=26·37 division.

$$1 \text{ division}=\frac{\cdot 3282}{26\cdot 37}=\cdot 01244.$$

Being interrupted, I could not continue the series of rider determinations further.

Determination of (B+X)−(A+Y).

Weight in pan.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences.
A+Y	291·7 275 289·5	282·80		
B+X	261·0 280·3 263·4	271·25	282·85	11·60
A+Y	285·7 280·4 285·1	282·90	271·61	11·29
B+X	280·2 265·3 277·5	272·07	283·90	11·83
A+Y	294·9 276·1 292·5	284·90	273·43	11·47
B+X	276·0 273·9 275·4	274·8	286·40	11·60
A+Y	267·3 305·4 273·5	287·90	275·11	12·79
B+X	278·5 272·9 277·4	275·42	289·28	13·86
A+Y	296·6 285·5 295·1	290·67		

Mean (B+X)−(A+Y)=12·06 divisions.

∴ (B+X)−(A+Y)=0·1500 milligramme.

Greatest deviation from the mean =1·8 division =0·01224 mgm.

The previous determination of (B+X)−(A+Y) was ·0718 mgm. The difference is too great, ·0782 mgm., to be accounted for by errors of experiment. There must have been some deposit on one of the weights, either of dust or moisture. I therefore took them out, cleaned and adjusted them by scraping B till nearly equal to A, and removing the wax from A.

V.—June 14.—Determination of Value of 1 Scale Division.

Rider on beam.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences due to R – L.
R	212·6 230·3 214·7	221·97		
L	252·3 242·5 251·2	247·12	222·16	24·96
R	225·0 220·5 223·6	222·35	246·67	24·32
L	238·6 252·8 240·7	246·22	221·87	24·35
R	224·9 218·7 223·3	221·40	245·98	24·58
L	249·7 242·2 248·9	245·75	221·26	24·49
R	226·8 216·7 224·3	221·12		

∴ mean R – L = 24·54 divisions.

$$\therefore\ 1\ \text{division} = \frac{0\cdot3282}{24\cdot54} = 0\cdot01337\ \text{milligramme.}$$

Determination of (B + Y) − (A + X).

Weight in pan.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences.
A + X	206·3 212·7 206·8	209·62	This was rejected as being so much higher than the rest.	
B + Y	209·0 206·4 208·6	207·6		
A + X	212·4 204·0 211·3	207·92	207·71	·21
B + Y	206·8 208·8 206·9	207·82	207·88	·06
A + X	211·8 204·4 210·8	207·85	207·64	·21
B + Y	209·1 206·1 208·6	207·47	207·86	·39
A + X	210·7 205·5 209·8	207·87	207·11	·76
B + Y	208·5 205·3 207·9	206·75	207·83	·78
A + X	209·7 205·1 208·9	207·20	206·45	·75
B + Y	203·3 208·7 203·9	206·15		

Mean (B + Y) − (A + X) = ·49 division = 0·00655 milligramme.
Greatest deviation from mean = ·43 division = 0·00575 mgm.

A and B had here been cleaned and B readjusted by scraping. A small vessel containing calcium chloride was put inside the balance to dry the air. This improved the action of the clamp, diminishing the cohesion.

VI.—June 17.—Determination of Value of 1 Scale Division.

Rider on beam.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences due to R−L.
R	210·4 214·3 210·7	212·42		
L	238·4 233·3 238·1	235·77	212·93	22·84
R	209·5 217·0 210·3	213·45	236·17	22·72
L	246·2 228·1 243·9	236·57	213·38	23·19
R	224·2 204 221·1	213·32	236·59	23·27
L	233·7 239·1 234·6	236·62	213·52	23·09
R	207·6 219·4 208·6	213·75		

Mean R−L=23·02 divisions.

$$\therefore 1 \text{ division} = \frac{\cdot 3282}{23\cdot 02} = \cdot 01425 \text{ milligramme.}$$

Determination of (B+X)−(A+Y).

Weight in pan.	Extremities of Oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences.
B+X	216·9 214·6 216·6	215·67		
A+Y	256·7 233·8 253·9	244·55	216·08	28·47
B+X	220·1 213·7 218·5	216·50	244·90	28·40
A+Y	254·8 236·7 252·8	245·25	217·07	28·18
B+X	221·5 214·5 220·1	217·65	245·48	27·83
A+Y	257·5 235·3 254·8	245·72	217·37	28·35
B+X	220·9 214·0 219·5	217·1	245·68	28·58
A+Y	246·8 244·5 246·8	245·65	216·83	28·82
B+X	223·7 210·5 221·6	216·57		

∴ mean (B+X)−(A+Y)=28·38 divisions =0·4043 milligramm

Greatest deviation from mean =·55 division =·00784 milligramm

VII.—June 18.—Determination of 1 Scale Division.

Rider on beam.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences due to R – L.
R	171·6 190·7 172·7	181·42		
L	210·3 225·3 212·4	218·32	180·47	37·85
R	189·2 171·7 185·5	179·52	217·54	38·02
L	223·2 210·8 222·3	216·77	179·04	37·73
R	185·0 173·2 182·9	178·57	216·17	37·60
L	220·2 211·3 219·5	215·57	178·22	37·35
R	189·1 168·8 184·8	177·87		

Mean R – L = 37·71 divisions.

$$\therefore \text{ 1 scale division} = \frac{\cdot 3282}{3771} = \cdot 00870 \text{ milligramme.}$$

Determination of (B+Y)−(A+X).

Weight in pan.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences.
B+Y	208·5 221·7 209·5	215·35		
A+X	246·9 214·4 244·1	229·95	216·32	13·73
B+Y	219·2 215·6 218·0	217·1	230·91	13·81
A+X	240·8 221·0 238·7	231·87	217·12	14·75
B+Y	226·1 209·4 223·7	217·15	232·24	15·09
A+X	221·8 242·3 224·1	232·62	216·96	15·66
B+Y	211·5 221·8 212·0	216·77	232·21	15·44
A+X	219·0 243·0 222·2	231·80		
B+Y	208·3 229·0 209·9	219·05	This sudden change of resting point must be due to some irregular disturbance. It is therefore rejected. It was slowly returning to nearly its former values.	

Mean (B+X)−(A+Y)=14·75 divisions =·12831 milligramme.

Greatest deviation from the mean =1·02 division =·0089 mgm.

The great difference between the result here and that in series V is probably due to deposit of dust. The new mirrors had to be fixed up just before the experiment began, and the doors were open for some time. At the conclusion of the weighing I found a good deal of dust on the weights.

VIII.—June 19.—Determination of 1 Scale Division.

Rider on beam.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Difference due to R−L.
L	235·8 222·3 233·3	228·42	This is so much higher than the rest, probably through being observed soon after I entered the room, that it is rejected.	
R	197 181·2 193·9	188·32		
L	228·2 222·7 228·0	225·40	188·24	37·16
R	197·4 180·7 193·9	188·17	225·46	37·29
L	233·6 218·0 232·5	225·52	187·63	37·89
R	191·7 183·3 190·1	187·10	225·28	38·18
L	230·2 220·3 229·4	225·05	187·35	37·70
R	193·8 182·5 191·8	187·60		

Mean R−L=37·64 divisions.

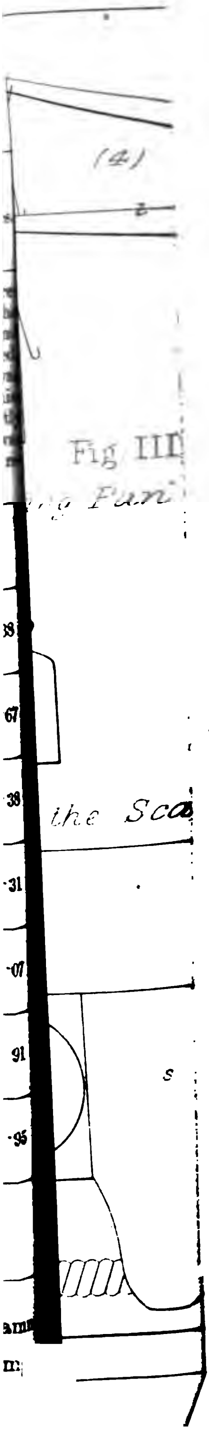
$$1 \text{ scale division} = \frac{\cdot 3282}{37 \cdot 64} = \cdot 00872.$$

Determination of (B+X)−(A+Y).

Weight in pan.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Diff
A+Y	245·3 238·7 244·2	241·72	In one observation r corded just before t clamp had been loc the scale pan had s and the resting poi thereby changed. T turbance had app not subsided when t taken, it is theref jected.	
B+X	193·0 185·7 191·4	188·95		
A+Y	226·6 243·3 229·2	235·60	188·22	4
B+X	182·2 192·8 182·4	187·55	235·22	4
A+Y	229·4 239·2 231·6	234·85	187·47	4
B+X	194·4 182·5 192·2	187·4	234·71	4
A+Y	239·8 229·7 239·1	234·57	187·50	4
B+X	194·7 181·6 192·5	187·60	234·51	4
A+Y	230·6 237·4 232·4	234·45	187·50	4
B+X	186·0 189·0 185·6	187·40		

Mean (B+X)−(A+Y)=47·24 divisions =0·4119 milligr

Greatest deviation from the mean =·43 division =·00375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Determination of (B+X)−(A+Y).

Weight in pan.	Extremities of oscillations.	Resting point.	Mean of preceding and succeeding resting points.	Differences.
A+Y	245·3 238·7 244·2	241·72	In one observation not recorded just before this the clamp had been loose, and the scale pan had slipped, and the resting point was thereby changed. The disturbance had apparently not subsided when this was taken, it is therefore rejected.	
B+X	193·0 185·7 191·4	188·95		
A+Y	226·6 243·3 229·2	235·60	188·22	47·38
B+X	182·2 192·8 182·4	187·55	235·22	47·67
A+Y	229·4 239·2 231·6	234·85	187·47	47·38
B+X	194·4 182·5 192·2	187·4	234·71	47·31
A+Y	239·8 229·7 239·1	234·57	187·50	47·07
B+X	194·7 181·6 192·5	187·60	234·51	46·91
A+Y	230·6 237·4 232·4	234·45	187·50	46·95
B+X	186·0 189·0 185·6	187·40		

Mean (B+X)−(A+Y)=47·24 divisions =0·4119 milligramme.

Greatest deviation from the mean =·43 division =·00375 mgm.

Fig. I

Plan

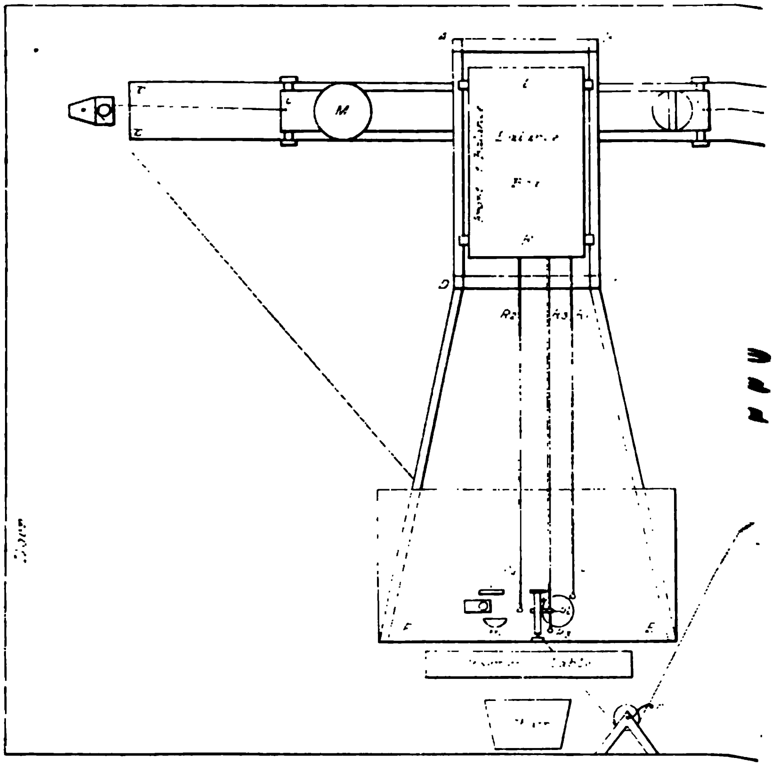
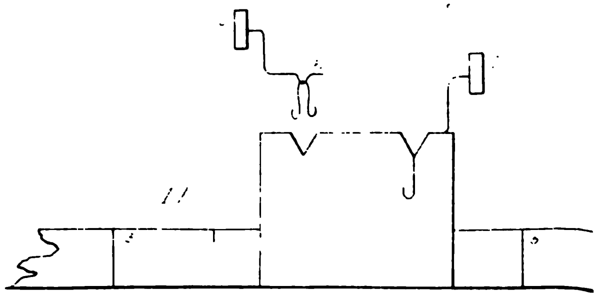


Fig. II

Apparatus to change the



Summary.

Series.			Mgms.	Greatest deviation from mean in milligrammes.	
1	(B+X)−(A+Y)	=	·0718	·0099	A=B+·0446 mgm.
2	(A+X)−(B+Y)	=	·1610	·0067	
3	(A+X)−(B+Y)	=	·1732	·0057	A=B+·0116 mgm.
4	(B+X)−(A+Y)	=	·1500	·0122	
5	(B+Y)−(A+X)	=	·0065	·0057	B=A+·1989 mgm.
6	(B+X)−(A+Y)	=	·4043	·0078	
7	(B+Y)−(A+X)	=	·1283	·0089	B=A+·1418 mgm.
8	(B+X)−(A+Y)	=	·4119	·0037	

The greatest error—that is the greatest deviation of any one value from the mean of its series—in the first four series is $\frac{1}{30000000}$th of a pound. The greatest error in the four series Nos. 5—8 is $\frac{1}{50000000}$th of a pound.

II. "On Repulsion resulting from Radiation." Part VI. By WILLIAM CROOKES, F.R.S., V.P.C.S.

(Abstract.)

In this part, with which the research closes, the author first examines the action of thin mica screens fixed on the fly of an ordinary radiometer, in modifying the movements. It is found that when a disk of thin clear mica is attached 1 millim. in front of the blacked side of the vanes of an ordinary radiometer, the fly moves negatively, the black side approaching instead of retreating from the light. When a thin mica disk is fixed on each side of the vanes of a radiometer, the result is an almost total loss of sensitiveness.

In order to examine the action of screens still further an instrument is described having the screens movable, and working on a pivot independent of the one carrying the fly, so that the screens can move freely and come close either to the black or to the white surfaces of the disks. By gentle tapping the screens can be brought within 2 millims. of the black surfaces. A candle is now brought near, shaded so that the light has to pass through one of the clear disks and fall on the black surface. The black side immediately retreats, the clear disk remaining stationary for a moment and then approaching the light. If the candle is allowed to shine on the plain side of the black disk, no immediate movement takes place. Very soon, however, both disks move in the same direction away from the candle, the speed of the clear disk gradually increasing over that of the blacked disk.

Instead of allowing the clear screens to freely move on a pivot, an instrument was made in which the screens could be fixed beforehand in any desired position in respect to the blacked disks. It was then found that with the screens close to the blacked sides of the vanes the fly rotates very slowly in the negative direction, stopping altogether when the candle is moved five or six inches off. With the screens 1 millim. from the black surface the direction is negative and the speed at its maximum. When the screens and disks are 7 millims. apart a position of neutrality is attained, no movement taking place. When the distance is further increased, positive rotation commences, which gets stronger as the screens approach the bright sides of the disks, where the positive rotation is at its maximum. The author adduces reasons for considering that the negative rotations here observed are caused by the warming up of the black surface by radiation falling direct on it, through the clear mica screen, and the deflection backwards of the lines of molecular pressure thereby generated.

The action of these radiometers being complicated, owing to the surfaces of the vanes being different in absorptive power, another instrument was made in which the vanes were of polished aluminium, perfectly flat and symmetrical with the bulb. The screens were of clear mica movable in respect to the vanes, and at right angles to their surface. When exposed to the light of a candle it was found that with the screens brought up close to the disks, the rotation was as if the unscreened side were repelled; at an intermediate position there was neutrality. Explanations are given of these movements, but without the illustrative cuts they would be unintelligible.

Experiments on radiometers having movable screens interposed between the vanes and the bulb are next given, and these are followed by a long series of experiments on the influence of movable screens on radiometers with cup-shaped metallic vanes, the screens being varied in shape, and position in respect to the plane of rotation, as well as in respect to the distance from the vanes.

A similar series is given with metallic cylinders as vanes, and from the behaviour of the latter kind of radiometer, an explanation is given of the various movements previously obtained. It is found that when the screen touches the convex surface of the vanes the rotation under the influence of light is always positive. It commences at a low exhaustion, increases in speed till the rarefaction is so high that an ordinary radiometer would begin to lose sensitiveness, and afterwards remains at about the same speed up to the highest rarefaction yet obtained. At any rarefaction after 87 M (millionths of an atmosphere) there is a neutral position for the screen. When it is on the concave side of this neutral position the direction of rotation is positive, and when on the convex side of the neutral position it is

negative; the speed of rotation is greater as the vanes are further removed from this neutral position on either side. The position of this neutral point varies with the degree of exhaustion; thus at 12 M, the screens must be 3 millims. from the convex side; at ·18 M they must be 13 millims. from the convex side. The higher the exhaustion the greater the distance which must separate the convex side of the hemi-cylinders and the screens.

The author gives explanations of these phenomena, based on the following already ascertained facts:—When thin aluminium vanes are exposed to light the metal rises in temperature and becomes equally warm throughout, and a layer of molecular pressure is generated on its surface. The thickness of this layer of pressure, or the length of the lines of force of repulsion, varies with the degree of exhaustion, being longer as the exhaustion increases. The lines of force appear to radiate from the metal in a direction normal to its surface. The force of repulsion is also greater the closer the repelled body is to the generating or driving surface, and the force diminishes rapidly as the distance increases, according to a law which does not appear to be that of "inverse squares." Diagrams are given illustrating the author's explanation, based on the above data.

An apparatus is next described not differing in principle from the last, but having, in addition to the aluminium hemi-cylinder and movable mica screen, a small rotating fly made of clear mica, mounted in such a way that it could be fixed by means of an exterior magnet in any desired position inside the bulb. The screen was also capable of adjustment by means of another magnet; the aluminium hemi-cylinder in this apparatus being fixed immovable. The adjustible indicator being very small in diameter in comparison to the other parts of the apparatus, and, being easily placed in any part of the bulb, was expected to afford information as to the intensity and direction of the lines of pressure when a candle was brought near the bulb. Experiments have been tried, *a*, with the screen in different positions in respect to the hemi-cylinder; *b*, with the indicator in different parts of the bulb; *c*, with the candle at different distances from the hemi-cylinder on one side or the other; *d*, with the degree of exhaustion varying between wide limits. It would be impossible to give an intelligible abstract of the results obtained with this apparatus without numerous diagrams. It may, however, be briefly stated that they entirely corroborate the theories formed from a study of the behaviour of the instruments previously described.

The next part of the paper treats of the action of heat employed inside the radiometer. In a previous paper, the author showed that phenomena feeble and contradictory when caused by radiation external to the bulb, became vigorous and uniform when the radiation was applied internally by the agency of an electrically-heated wire. It

was hoped that some of the more obscure phenomena shown by the deep cups with movable screens in front (referred to above) might be intensified if set in action by a hot wire. Several kinds of apparatus and experiments with them are described, but the results are too complicated to be given in abstract. One experiment proves that the direction of pressure is not wholly normal to the surface on which it is generated, but that some of it is tangential.

The author then describes the turbine radiometer, early specimens of which were exhibited before the Royal Society on April 5, 1876. In the ordinary form of radiometer the number of disks constituting the fly is limited to six or eight, a greater number causing interference one with the other and obstruction of the incident light. In the turbine form of fly there is no such difficulty, the number of vanes may be considerably increased without overcrowding, and with corresponding advantage. In the earlier turbine radiometers the flies were made of mica blacked on both sides, and inclined at an angle like the sails of a windmill, instead of being in a vertical plane. This form of instrument is not sensitive to horizontal radiation, but moves readily in one or other direction to a candle held above or below. A vertical light falling on the fly gives the strongest action, but rotation takes place, whatever be the incident angle, provided the light is caught by one surface more than by the other. Ether dropped on the top of the bulb to chill it causes rapid negative rotation. If the turbine radiometer is floated in a vessel of ice-cold water, and the upper portion exposed to the air of a warm room, it rotates rapidly in the positive direction, acting as a heat engine, and continuing so to act until the rotating fly has equalised the temperature of the upper and lower portions of the bulb. By reversing the circle of operations—by floating the turbine radiometer in hot water and cooling the upper portion of the bulb—the fly instantly rotates in the negative direction.

After describing experiments in which the same fly was made to rotate first in a large bulb and then in a small one at the same degree of exhaustion, the author proceeds to discuss the influence exerted by the inner side of the glass case of the radiometer as a reacting surface. A flat metal band was put equatorially inside a radiometer, and lampblacked, so that the molecular pressure generated under the influence of light should react between the fly and the black band, instead of between the fly and the glass side of the bulb. It was found that the maximum speed with the band present was 40 revolutions a minute, against $8\frac{1}{4}$ revolutions when the band was absent.

The rotation of the case of a radiometer, the fly being held immovable by magnetism, is next described. A preliminary note on this subject having already appeared in the "Proceedings,"* it need not be again

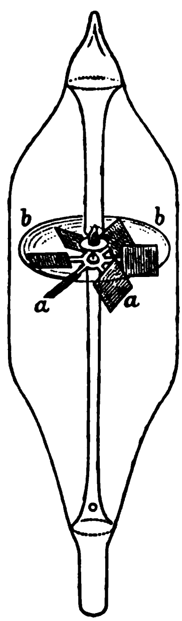
* "Proc. Roy. Soc.," No. 168, March 30, 1876.

described in detail. Many different forms of instrument for effectin this rotation are described, and their mode of action explained.

The reacting inner surface of the envelope being thus proved to l essential to the rotation of the fly, other instruments were made i which this necessary reaction is obtained in a more direct manne In one, the radiometer is furnished with a fly carrying four fl aluminium vanes, polished on both sides. Three vertical partitio of thin clear mica are fixed in the bulb, with their planes not passin through the axis of rotation, but inclined to it, thus throwing tl obliquity off the fly on to the case, and giving three fixed planes fo the reaction to take place against. Candles arranged symmetrical round the bulb make the fly rotate rapidly against the edges of tl inclined planes. Breathing gently on the bulb gives negative rotatio A hot glass shade inverted over the instrument causes strong negati rotation, changing to positive on cooling. When the fly is furnishe with clear mica or with silver flake mica vanes, the same results a obtained as when aluminium vanes are employed. The princip action is produced by dark heat warming the bulb, screens, and vane

The *otheoscope* is the next subject treated on in the paper. This h already been given in abstract,* and need not be again referred t Many different varieties of otheoscope are figured and described.

FIG. 1.



* "Proc. Roy. Soc.," No. 180, April 26, 1877.

It was suggested by Professor Stokes that a disk might be made to revolve on its axis, and the author describes an instrument in which this suggestion is carried out. The disk is horizontal, mounted like the fly of a radiometer, and for lightness' sake is of mica, blacked above. Fixed to the bulb above the disk are four flat pieces of clear mica; each extends from the side of the bulb to near the centre, and ends below in a straight horizontal edge, leaving just space enough for the disk to revolve without risk of scraping. The edge is in a radial direction, and the plane of the plates is inclined about 45° to the horizon, in the same direction for them all. Exposed to the light of a candle, the rotation is against the edge. By slightly modifying this form, the instrument becomes much more sensitive. Fig. 1 shows the complete instrument; *a*, *a*, are six vanes of copper foil, oxidised by heating to redness in the air; they are attached to arms, and are inclined at an angle of 45° to the horizon. They are firmly fixed to the support. Through the centre passes a needle-point, on which is balanced a glass cup, carrying a thin clear disk of mica, *b*, *b*, freely rotating about 1 millim. above the top edges of the copper vanes. When exposed to light, the mica disk rotates with great speed against the edges. The pressure which drives the movable fly round reacts equally on the driving surface: by suspending both vanes and disk independently on needle-points the effect of light causes them to rotate in opposite directions.

Whilst experimenting with the otheoscope it was found that, for a given exhaustion, the nearer the reacting surfaces were together the greater was the speed obtained. In the "Proceedings of the Royal Society" for November, 1876,* the author described an apparatus by which he was able to measure the thickness of the layer of molecular pressure generated when radiation impinged on a blackened surface enclosed in an atmosphere the rarefaction of which could be varied at will.

It was found that in this apparatus repulsion could be obtained at ordinary atmospheric pressures. Observations are given at normal pressure and at various degrees of rarefaction, with the driving and moving surfaces separated 1, 2, 3, 4, 6, 8, and 12 millims.; and diagrams of the resulting curves are shown when the atmospheric tension and the force of repulsion are used as abscissæ and ordinates. The tables and curves show that the law of increase of the force with the diminution of the distance between the disks does not remain uniform at all rarefactions. At the lowest exhaustions the mean path of the molecules of the attenuated gas is less than 1 millim., as rendered evident by the force of repulsion diminishing rapidly as the distance increases. At exhaustions higher than 9 millims. this condition alters, and as the

* "Proc. Roy. Soc.," No. 175, vol. xxv, p. 310.

gauge approaches barometric height, the molecular pressure tends to become uniform through considerable distances, the mean path of the molecules now being comparable with the greatest distance separating the surfaces between which they act.

A similar apparatus to the one in which the last experiments were tried was used to measure the action at pressures at and approaching atmospheric. At pressures between atmospheric and 210 millims., the first action is very faint repulsion, immediately followed by strong attraction. The attraction then begins to decline, until at 15 millims. pressure it disappears. At the same time the repulsion, which begins to be apparent at 250 millims., increases as the attraction diminishes. The author considers that the attraction is the result of air-currents, caused by the permanent heating of the surface in front of the moveable disk.

The paper concludes with experiments undertaken to measure the amount of repulsion, using a horizontal torsion balance,* on the principle of Ritchie's, in which the force of repulsion is balanced by the torsion of a fine glass fibre. The *pan* of the balance is a clear mica disk, and a similar disk is fastened to the tube in which the beam oscillates. This fixed disk is lampblacked on the upper side, and beneath is a spiral of platinum wire, connected with terminals sealed through the side of the tube. When the spiral is ignited by a constant electric current, the blacked mica disk fixed above it becomes heated, and the molecular pressure thereby generated between it and the mica pan causes the latter to rise. The glass thread attached to the beam is thus twisted, and by means of a graduated circle the number of degrees through which the thread has to be turned in order to bring the beam back to equilibrium is noted. This gives a measurement of the pressure exerted, in torsional degrees, and these are converted into grains by ascertaining how many torsional degrees correspond to a known weight. A ray of light reflected from a mirror in the centre of the beam is used as an index, being brought back to zero at each experiment. The author gives in a table, and also shows in the form of a curve, the results obtained with this apparatus, giving the force of molecular pressure in grains weight at exhaustions varying between 2,237 and 0·7 millionths of an atmosphere.

* For a description of this form of torsion balance, see the author's paper, "Phil. Trans.," 1876, vol. clxvi, p. 371.

November 30, 1878.

ANNIVERSARY MEETING.

Sir JOSEPH HOOKER, C.B., K.C.S.I., President, in the Chair.

General Boileau, for the Auditors of the Treasurer's Accounts on the part of the Society, reported that the total ordinary receipts during the past year, including a balance of £933 11*s*. 1*d*. carried from the preceding year, amount to £5,924 5*s*. 9*d*., and that the total ordinary expenditure in the same period amounts to £5,008 1*s*. 2*d*., leaving a balance at the Bankers of £894 2*s*. 3*d*., and £22 2*s*. 4*d*. in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

On the Home List.

Admiral Sir George Back, D.C.L.
Edward Blackett Beaumont.
Rev. James Booth, LL.D.
Lieut.-General John Cameron, R.E., C.B.
Frederick, Lord Chelmsford, D.C.L.
Rev. William B. Clarke, M.A.
Thomas Grubb, M.R.I.A.
Right Hon. Russell Gurney, Q.C.
Rear-Admiral Sir William Hutcheon Hall, K.C.B.
Prof. Robert Harkness, F.G.S.
John Hilton, F.R.C.S.
Cuthbert William Johnson.
Rev. Robert Main, M.A.
Colonel Thomas George Montgomerie, R.E.
Thomas Oldham, M.A., LL.D.
John Penn.
John, Earl Russell, K.G.
Very Rev. Augustus Page Saunders, D.D., Dean of Peterborough.
William Stokes, M.D., D.C.L.
Thomas Thomson, M.D.
Major-General Sir Andrew Scott Waugh, R.E.

On the Foreign List.

Antoine César Becquerel.
Claude Bernard.
Elias Magnus Fries.
Henri Victor Regnault.
Angelo Secchi.
Ernst Heinrich Weber.

Fellows elected since the last Anniversary.

John Gilbert Baker, F.L.S.
Francis Maitland Balfour, M.A.
Rev. Thomas George Bonney, M.A.
Prof. James Henry Cotterill, M.A.
Sir Walter Elliot, K.C.S.I.
Rev. Canon W. Greenwell, M.A.
Right Hon. Sir William Henry Gregory, K.C.M.G.
Thomas Hawksley, C.E.
John Hopkinson, M.A., D.Sc.
John Hughlings Jackson, M.D.
Lord Lindsay, P.R.A.S.
Samuel Roberts, M.A.
Edward A. Schäfer, M.R.C.S.
Right Hon. William Henry Smith.
Hermann Sprengel, Ph.D.
George James Symons.
Charles S. Tomes, M.A.

On the Foreign List.

Marcellin Berthelot.
Joseph Decaisne.
Emil Du Bois Reymond.
Adolph Wilhelm Hermann Kolbe.
Rudolph Leuckart.
Simon Newcomb.
Pafnutij Tchebitchef.

The President then addressed the Society as follows :—

GENTLEMEN,

AT the conclusion of this, the fifth and last year during which I shall have held the most honorable office of your President, I have the gratifying assurance that the communications made to the Society and its publications have in no respect fallen off in scientific interest and value. We have not, indeed, been called upon to undertake during the past year such responsible and time-absorbing duties in behalf of the Government as the Polar, Circumnavigation, Transit of Venus, and other Committees demanded of us during the previous four years; but some of the results already achieved by those expeditions have been contributed to our publications, and we are in expectation of more. It is also with satisfaction that I can refer to the good attendance at our evening meetings, soirées, and réunions as evidence of the interest taken in our proceedings by the Fellows generally and their friends.

Before proceeding to touch upon some of the advances made in Science during the last few years, I have, as heretofore, to inform you of the Society's condition and prospects, and of those duties undertaken by its Council, for information as to which non-resident Fellows look to the annual address.

The loss by death of Fellows, twenty-one in number, is but little short of last year's rate, while that of Foreign Fellows (six) is twice as great as last year. On the home list is Sir George Back, the last, with

the exception of our former President, the venerable Sir E. Sabine, of that celebrated band of Arctic voyagers, which during the early part of the century added so much to our renown as navigators and discoverers. Sir George was further the companion of Franklin and Richardson in that overland journey to the American Polar Sea, in which human endurance was tried to the uttermost compatible with human existence, as is related by two of the party in that modest but thrilling narrative which will ever hold a unique place in the annals of geographical discovery. Of our Indian explorers four have been taken away, namely, Major-General Sir Andrew Waugh, for many years Director of the Great Trigonometrical Survey of India; and shortly afterwards his friend, Col. Montgomerie; Dr. Oldham, for a quarter of a century the Director of the Geological Society of India; and Dr. Thomas Thomson, my fellow-traveller in the Himalaya, whose report of explorations in Western Tibet contains the first connected account of the physical and natural features of that remote and difficult country. Lieut.-General Cameron survived but for one year our late Fellow, Sir Henry James, his predecessor in the Direction of the Ordnance Survey of Great Britain. In the Rev. James Booth we have lost a mathematician of high attainments. The Rev. W. B. Clarke, of New South Wales, was the author of many papers on the Meteorology and Geology of the Cape of Good Hope, Australia, and the Pacific. The Rev. R. Main, Director of the Radcliffe Observatory, was for nearly half a century an indefatigable observer. Lastly, Earl Russell, the distinguished statesman, and the earnest advocate, whether in the Government or in Parliament, of every measure for the promotion of scientific inquiry. He it was who, when Prime Minister in 1849, wrote to the then Earl of Rosse, President of the Society, offering to place £1,000 (now known as the Government Grant) on the annual votes of Parliament, if the Council would undertake to apportion that sum among scientific workers requiring aid in their researches.

Of Foreign Fellows our losses are a great Chemist in Becquerel, of Paris, whose election took place upwards of forty years ago; a great Physiologist in Claude Bernard, also of Paris; the father of Mycology, and for long the patriarch of Scandinavian Botanists, Elias Fries; a most distinguished Physicist and the recipient of both a Rumford and Copley medal in Regnault; a veteran Anatomist in Weber; and in Secchi, of Rome, an Astronomer of astonishing activity, the author of more than three hundred separate contributions to the science of which he was so great an ornament.

In matters of Finance I may with satisfaction refer you to our Treasurer's Balance Sheets.

It will be in your recollection that Mr. T. J. Phillips Jodrell placed in 1874 a sum of £6,000 at the disposal of the Society, with the view

of its being devoted to the encouragement of Scientific Research by periodical grants to investigators whom your Council might think it expedient thus to aid. Shortly after the receipt of this munificent gift, the Government announced its intention of devoting annually for five years £4,000 to the same object, thus anticipating the special purpose which Mr. Jodrell had in view. Thereupon, with his consent, the donation was temporarily funded and the proceeds applied to the general purposes of the Society until some other scheme for its appropriation should be approved. In April last I received a further communication from Mr. Jodrell, declaring it to be his wish and intention that, subject to any appropriation of the sum which we might, with the approval of the Society, make during his lifetime, it should immediately on his death be incorporated with the Donation Fund, the annual income in the meantime going to the general revenue of the Society. Upon this subject I have now to state that since the receipt of that letter Mr. Jodrell has approved of £1,000 of the sum being contributed to a fund presently to be mentioned.

I have also to inform you of a cheque for £1,000 having been placed in my hands by our Fellow, Mr. James Young, of Kelly, to be expended in the interests of the Society in such manner as I should approve.

Mr. De La Rue, to whose beautiful experiments I shall have occasion to refer, has presented to the Society both the letterpress and the exquisitely engraved fac-similes of the electric discharges described in his and Dr. Hugo Müller's paper, recently published in our "Transactions."

Our Fellow, Dr. Bigsby, has presented seven copies of his "Thesaurus Devonico-carboniferus" for distribution, and they have been distributed accordingly.

A very valuable addition to our Gallery of deceased Fellows has been the gift by Mr. Leonard Lyell of a copy in marble by Theed of the bust of his uncle the late Sir Charles Lyell, F.R.S., together with a pedestal. This is the best likeness of the late eminent geologist that has been executed, and is in every respect a satisfactory one.

I have the gratification of announcing to you, that through the munificence of a small number of Fellows, means have been advanced for reducing the fees to which all ordinary Fellows in future elected will be liable. That these fees, though not higher than the most economical expenditure on the part of the Society for its special purposes demanded, were higher than it was expedient to maintain if any possible means for reducing them could be obtained, was not only my own opinion but that of many Fellows. They exceed those of any other scientific society in England or abroad; their amount has

occasionally prevented men of great merit from having their names brought forward as candidates, and they press heavily, especially upon those who, with limited incomes, have other scientific societies to subscribe to. Nor does it appear to me as otherwise than regrettable that so high an honour as Fellowship of the Royal Society, the only one of the kind in England that is limited as to the number annually elected, and selective in principle, should be attainable only at a heavy pecuniary expenditure. It is true that our Fellows receive annually in return publications of great value to Science generally; but these treat of so many branches of knowledge that it is but a fraction of each that can materially benefit the recipient, while their bulk entails an additional expenditure; and now that the individual papers published in the "Transactions" are separately obtainable, the advantages of Fellowship are less than they were when to obtain a treatise on his own subject a specialist had either to join the Society, or to purchase a whole volume or a large part of it annually.

It was not, however, till I had satisfied myself that the annual income of the Society, though not ample, was sufficient for its ordinary purposes, that its prospects in other points of view were good, and that the expenditure upon publication was the main, if not the sole, obstacle to a reduction of fees, that I consulted your Treasurer on the subject of taking steps to attain this object.

My first idea was to create, by contributions of small amount, a fund the interest of which should be allowed to accumulate; and when the income of the accumulated capital reached a sufficient amount to enable the Society to take the step without loss of income, to reduce either the entrance fee or annual contribution; and to which fund Mr. Young's gift should be regarded as the first donation.

This proposal was in so far entertained by your Council that they resolved to establish a Publication Fund, and to place Mr. Young's gift to the credit thereof; and further, appointed a Committee to consider and report upon the Statutes of the Society concerning the fees.

The movement once set on foot met with an unexpectedly enthusiastic reception, several Fellows with the best means of forming a judgment, not only approved of it, but offered liberal aid, urging that the reduction of fees should be the first and immediate object, and that if such a course were thought desirable, the means of carrying it out would surely be forthcoming. On this your Treasurer prepared for my consideration a plan for raising £10,000, the sum required for effecting any material reduction; and we resolved to ascertain by private inquiry whether so large an amount could be obtained.

Here again our inquiries were responded to in a spirit of, I may say, unexampled liberality: in a few weeks upwards of £8,000 was given

or promised by twenty Fellows of the Society, and I need hardly add that the remaining £2,000 was contributed very shortly afterwards.

At a subsequent meeting of the Council it was resolved:—

1.—That the sums referred to as the Publication Fund, as well as those received or that may be hereafter received, for the purpose of relieving future ordinary Fellows from the Entrance Fee, and for reducing their Annual Contribution, be formed into one fund.

2.—That the Entrance Fee for ordinary Fellows be henceforth abolished; and that the Annual Contribution for ordinary Fellows hereafter elected be £3 (three pounds). Also, that the income of the Fund above-mentioned be applied, so far as is requisite, to make up the loss to the Society arising from these remissions and reductions.

3.—That the account of this Fund be kept separate; and that the annual surplus of income, after providing for the remission and reduction above recommended, be re-invested, until the income from the Fund reaches £600. So soon as the annual income reaches this amount, any surplus of income in any year, after providing for the remission and reduction above-mentioned, shall be available, in the first instance, in aid of publication and for the promotion of research.

A list of subscribers to this Fund will be placed in the hands of every Fellow, with the information that it will be kept open for future contributions, in the interests of research and of the Society's publications. I hope that it will be largely and speedily augmented, and that it may eventually reach an amount which will provide us with the means of accomplishing as much as is effected by the Government Fund, upon our own sole and undivided responsibility. I must not conclude my notice of this movement without a mention of those whose encouragement and liberality have most largely promoted it; and first of all, Mr. Spottiswoode, to whose counsel and active co-operation throughout, its success is mainly due; Messrs. Young's and Jodrell's contributions have already been mentioned, they have been supported by others:—£2,000 from Sir Joseph Whitworth, £1,000 from Sir W. Armstrong, and £500 each from His Grace the Duke of Devonshire, Mr. De La Rue, Mr. Spottiswoode and Mr. Eyre (jointly), Dr. Siemens, and the Earl of Derby, and £250 from Dr. Gladstone. The balance comprises contributions by thirty-two Fellows.

I have to mention your obligations to Dr. W. Farr for the labour he has bestowed in ascertaining those vital and other statistics of the Society, upon an accurate knowledge of which the calculations for the reduction of fees had to be based; and to Mr. Bramwell for constructing a table showing to what extent these changes will affect the

Society's present and future income. It may interest you to know that the contribution of ordinary Fellows in future to be elected, is but little over that which was required of all Fellows from the very commencement of the Society's existence till 1823, namely, 1*s.* per week, and that the last Fellows who paid that sum died in 1869.

Looking back over the five years during which I have occupied this chair, I recognise advances in scientific discovery and research at home and abroad far greater than any previous semi-decade can show. I do not here allude to such inventions as the Telephone, Phonograph, and Microphone, wonderful as they are, and promising immediate results of great importance to the community; nor even to those outcomes of high attainments, the Harmonic Analyser of Sir W. Thomson, and the Bathometer and Gravitation Meter of Siemens; but to those discoveries and advances which appeal to the seeker of knowledge for its own sake, whether as developing principles, suggesting new fields of research, or awakening attention to hitherto unseen or unrecognised, or unexplained phenomena of nature, and of which the Radiometer and Otheoscope of Crookes are conspicuous examples.

In the foremost rank as regards the magnitude of the undertakings and the combination of means to carry them out, nothing in the history of physical science can compare with the Transit of Venus Expeditions. To observe the Transit of Venus various nations of Europe and the United States competed as to the completeness of the Expeditions they severally equipped. The value* of the solar parallax cannot be ascertained until the results of all the Expeditions are taken into account, when it will have an international claim to acceptance. But advances in this direction will not have ended here, the very difficulties attending the observation of the Transit of Venus, having directed attention to the method originally suggested by the Astronomer Royal in 1857, of obtaining the solar parallax from the diurnal parallax of Mars at its opposition.

Mr. Gill by the skilful employment at Ascension Island of the heliometer lent by Lord Lindsay, has greatly increased the accuracy of the method by which the necessary star comparisons with Mars are made, and there is every reason to believe that the results of his observations which are now in course of reduction will be very satisfactory.

Within the last two years a remarkable addition has been made to

* The Astronomer Royal informs me that Captain Tupman, who has taken the principal share in the superintendence of the calculation, fixes provisionally on a mean parallax of 8″·8455, corresponding to a distance of 92,400,000 British miles, but that the observations would be fairly satisfied by any parallax between 8″·82 and 8″·88, which in distance produces a range of from 92,044,000 and 92,770,000 miles, differing by 726,000 miles, a quantity almost equal to the sun's diameter.

the number of members of the solar system by Professor Asaph Hall's discovery of the satellites of Mars; and more recently, Professor Watson has announced his detection of planetary bodies within the orbit of Mercury, during the Solar Eclipse which was visible in America.

In 1876 Schmidt recorded an outburst of light in a star in Cygnus, which showed a continuous spectrum containing bright lines similar to those of the remarkable star of 1866. As the star waned the continuous spectrum and bright lines faded, all but one bright line in the green, giving the object the spectroscopic appearance of a small gaseous nebula.

Great progress has been made during the last five years at Greenwich in the method of determining the motions of the heavenly bodies by the displacement of the lines in their spectra, as first successfully accomplished by Mr. Huggins in 1868. Not only do the results obtained by the stars observed at Greenwich agree with those of Mr. Huggins, as satisfactorily as can be expected in so delicate an investigation, but the motions of seventeen more have been determined; while the trustworthiness of the method has been shown by the agreement of the values for the rotation of the sun and the motions of Venus, with the known movements of these bodies. Mr. Huggins has also obtained photographs of the spectra of some of the brighter stars, which give well defined lines in the violet and ultra-violet parts of the spectrum. These spectra have already shown alterations in the lines common to them and the sun, which are of much interest.

In Solar Physics, which afford remarkable evidence of Mr. Lockyer's energetic labours in this country and Mr. Janssen's in France, I must mention our Foreign Member's wonderful photographs of the sun, wherein the minutest of the constant changes in the granulations exhibited on its surface (and which vary in size from $\frac{1}{10}$ of a second to 3 or 4 seconds) can be studied in future from hour to hour and day to day; as can also their different behaviour at different periods of the occurrence of sun-spots.

Before dismissing this fruitful field of research, I must allude to Mr. Lockyer's discovery of carbon in the sun; and to his announced but not yet published observations on the changes and modifications of spectra under different conditions, some of which he even regards as indicating the breaking up of the atoms of bodies hitherto regarded as elementary.

Some important investigations on the Electric Discharge have been communicated to the Society by Messrs. De La Rue and Müller, and by Mr. Spottiswoode. These, prosecuted by different means, tend to limit the possible causes of the stratification observed in discharges through vacuum tubes. They also point to the conclusion that this

phenomenon is in a great measure due to motions among the molecules of the residual gas, which themselves become vehicles for the transmission of Electricity through the tube. It is well known that gases at atmospheric pressure offer great resistance to the passage of Electricity; and that this resistance diminishes (to a certain limit, different for different gases) with the pressure. And the researches in question appear to show that the discharge, manifestly disruptive at the higher pressures, is really also disruptive even at pressures when stratification takes place. The period of these discontinuous discharges has not yet been the subject of measurement, but it must, in any case, be extremely rapid.

The remarkable experiments which have resulted in the liquefaction of the gases hitherto regarded as permanent will be noticed presently when I deliver to their authors the medals they so richly deserve.

Under the auspices of the Elder Brethren of the Trinity House, and as their scientific adviser, Professor Tyndall has conducted an investigation on the acoustic properties of the atmosphere. The instruments employed included steam whistles, trumpets, steam syrens, and guns. The propagation of sound through fog was proved to depend not upon the suspended aqueous particles, but upon the condition of the sustaining air. And as air of great homogeneity is the usual associate of fog, such a medium is often astonishingly transparent to sound. Hail, rain, snow, and ordinary misty weather, were also proved to offer no sensible obstruction to the passage of sound. Every phenomenon observed upon the large scale was afterwards reproduced experimentally. Clouds, fumes, and artificial showers of rain, hail, and snow were proved quite ineffectual to stop the sound, so long as the air was homogeneous, while the introduction of a couple of burners into a space filled with acoustically transparent air soon rendered it impervious to the waves of sound. As long as the continuity of the air in their interstices was preserved, the sound-waves passed freely through silk, flannel, green baize, even through masses of hard felt half an inch in thickness, the same sound-waves being intercepted by goldbeater's skin. A cambric handkerchief which, when dry, offered no impediment to their passage, when dipped into water became an impassable barrier to the sound-waves.

Echoes of extraordinary intensity were sent back from non-homogeneous transparent air; while similar echoes were afterwards obtained from the air of the laboratory, rendered non-homogeneous by artificial means. Detached masses of non-homogeneous air often drift through the atmosphere, as clouds pass over the face of the sky. This has been proved by the fluctuations observed with bells having their clappers adjusted mechanically, so as to give a uniform stroke. The fluctuations occur only on certain days; they occur when care has been taken to perfectly damp the bell between every two suc-

ceeding strokes; and they also occur when the direction of the sound is at right angles to that of the wind. Numerous observations were also made on the influence of the wind, the results obtained by previous observers being thereby confirmed. From his own observations, as well as from the antecedent ones of Mr. Alexander Beazeley and Professor Osborne Reynolds, Professor Tyndall concludes that the explanation of this phenomenon given by Professor Stokes is the true one.

Turning now to biological branches of Science, I find that the discoveries and researches of the past five years in this department also are far in advance of those of any previous period of equal length. The "Challenger" Expedition was, in point of the magnitude of the undertaking and completeness of its equipment, the rival of that for observing the Transit of Venus. Its general results, as far as hitherto made known, have been dwelt upon in my previous addresses, and the publication of them in detail is being rapidly pushed forward. Some very important papers by Mr. Moseley on the Corals collected on the voyage have indeed been published in our "Transactions" with admirable illustrations by himself.

To the Botanist and Geologist no subject has a greater interest than that of the conditions under which the successive Floras, which inhabited the polar area, existed and were successively dispersed over lower latitudes previous to their extinction, some *in toto* and over the whole globe, while others, though extinct in the regions where they once flourished, exist now only in lower latitudes under identical or under representative forms. It is only during the last few years that, thanks to the labours of those engaged in systematic Botany in tracing accurately the directions of migrations of existing genera and species, and in determining the affinities of the extinct ones, and of Palæontologists in referring the latter to their respective geological horizons, that any material advance has been made towards a knowledge of the origin and distribution of earlier and later Floras. I cannot better illustrate the condition of this inquiry than by calling your attention to two publications on the subject, which have appeared within the last few months.

As a contribution to the principles of Geographical Botany, Count Gaston de Saporta's essay, entitled "L'Ancienne Végétation Polaire" (which appeared in the "Comptes Rendus" of the French International Geographical Congress) is a very suggestive one, and having regard especially to its author's eminence as a geologist and palæontologist, is sure to command attentive study. Although it may be argued that neither is solar nor terrestrial physics, nor Geology, nor Palæontology in a sufficiently advanced condition to warrant the acceptation as fully established truths of all the conclusions therein advanced, still the array of facts adduced in evidence of these conclusions is very im-

posing, while the ability and adroitness with which they are brought to bear on the subject are almost worthy of the great French genius whose speculations form the starting-point of the theory, which is that life appeared first in the northern circumpolar area of the globe, and that this was the birthplace of the first and of all subsequent Floras.

I should premise that Count Saporta professedly bases his speculations upon the labours of his friend, Professor Heer, whose reasonings and speculations he ever puts forward with generous appreciation, while differing from him wholly on the subject of evolution, of which he is an uncompromising supporter; Professor Heer holding to the doctrine of the sporadic creation of species.

In his "Epoques de la Nature" Buffon argues that the cooling of the globe, having been a gradual process, the polar regions must have been the first in which the heat was sufficiently moderate for life to appear upon it; that other regions being as yet too hot to give origin to organised beings, a long period must have elapsed, during which the northern regions, being no longer incandescent, as they and all others originally were, must have had the same temperature as the tropical regions now possess.

Starting from this thesis, Count Saporta proceeds to assume that the termination of the Azoic period coincided with a cooling of the water to the point at which the coagulation of albumen does not take place; and that then organic life appeared, not in contact with the atmosphere, but in the water itself. Not only does he regard life as originating, if not at the North Pole, at least near to it, but he holds that for a long period life was active and reproductive only there. In evidence of this he cites various geological facts, as that the older, and at the same time the richest, fosilliferous beds are found in the cool latitudes of the North, namely in lats. 50° to 60°, and beyond them. It is in the North, he says, that Silurian formations occur, and though they extend as far south as lat. 35° N. in Spain and America, the most characteristic beds are found in Bohemia, England, Scandinavia, and the United States. The Laurentian rocks again, he says, reach their highest development in Canada, and Palæozoic rocks cover a considerable polar area north of the American great lakes, and appear in the coasts of Baffin's Bay, and in parts of Greenland and Spitzbergen. It is the same with the Upper Devonian and marine carboniferous beds preceding the coal formations; these extend to 76° N. in the polar islands and in Greenland, and to 79° N. in Spitzbergen, and he adds that M. d'Archiac has long ago remarked that, though so continuous to the northward, the coal-beds become exceptional to the southward of 35° N. Hence Count Saporta concludes that the climatic conditions favourable to the formation of coal were not everywhere prevalent on the globe, for that while the southern limit of this forma-

tion may be approximately drawn, its northern must have extended to the Pole itself.

I pass over Saporta's speculations regarding the initial conditions of terrestrial life, which followed upon the emergence of the earlier stratified rocks from the Polar Ocean, and proceed to his discussion of the climate of the carboniferous epoch as indicated by the characters of its vegetation, and of the conditions under which alone he conceives this can have flourished in latitudes now continuously deprived of solar light throughout many months of the year. In the first place, he accepts Heer's conclusions (founded on the presence of a tree-fern in the coal measures specifically similar to an existing tropical one), that the climate was warm, moist, and equable, and continuously so over the whole globe, without distinction of latitude. This leads him to ask whether, when the polar regions were inhabited by the same species as Europe itself, they could have been exposed to conditions which turned their summers into a day of many months' duration, and their winters into a night of proportional length?

A temperature so equable throughout the year as to favour a rich growth of Cryptogamic plants, appears, he says, to be at first sight incompatible with such alternating conditions as a winter of one long night and a summer of one long day; but equability, even in high latitudes, may be produced by the effect of fogs due to southerly warm oceanic currents, such as bathe the Orkneys and even Bear Island (in lat. 75° N.), and render their summers cool and winters mild. To the direct effects of these he would add the action of such fogs in obstructing terrestrial radiation, and hence preventing the evil effects which its cold would otherwise induce; and he would further efface the existing conditions of a long winter darkness by the hypothesis that the solar light was not, during the formation of the coal, distributed over the globe as it now is, but was far more diffusive, the solar body not having yet arrived at its present state of condensation.

That the polar area was the centre of origination for the successive phases of vegetation that have appeared in the globe is evidenced, under Count Saporta's view, by the fact that all formations, Carboniferous, Jurassic, Cretaceous, and Tertiary, are alike abundantly represented in the rocks of that area, and that, in each case, their constitutents closely resemble that of much lower latitudes. The first indications of the climate. cooling in these regions is afforded by *Coniferæ*, which appear in the polar lower Cretaceous formations. These are followed by the first appearance of Dicotyledons with deciduous leaves, which again marks the period when the summer and winter season first became strongly contrasted. The introduction of these (deciduous-leaved trees) he regards as the greatest revolution in vegetation that the world has seen; and he conceives that once evolved they increased, both in multiplicity and in diversity of form,

with great rapidity, and not in one spot only, and continued to do so down to the present time.

Lastly, the advent of the Miocene period, in the polar area, was accompanied with the production of a profusion of genera, the majority of which have existing representatives which must now be sought in a latitude 40° farther south, and to which they were driven by the advent and advance of the glacial cold; and here Count Saporta's conclusions accord with those of Professor A. Gray, who first showed, now twenty years ago, that the representatives of the elements of the United States Flora previously inhabited high northern latitudes, from which they were driven south during the Glacial period.

Perhaps the most novel idea in Count Saporta's Essay is that of the diffused sunlight which (with a densely clouded atmosphere), the author assumes to have been operative in reducing the contrast between the polar summers and winters. If it be accepted it at once disposes of the difficulty of admitting that evergreen trees survived a long polar winter of total darkness, and a summer of constant stimulation by bright sunlight; and if, further, it is admitted that it is to internal heat we may ascribe the tropical aspect of the former vegetation of the polar region, then there is no necessity for assuming that the solar system at those periods was in a warmer area of stellar space, or that the position of the poles was altered, to account for the high temperature of Pre-Glacial times in high northern latitudes; or, lastly, that the main features of the great continents and oceans were very different in early geological times from what they now are. Count Saporta's views in certain points coincide with those of Professor Le Conte of California, who holds that the uniformity of climates during earlier conditions of the globe is not explicable by changes in the position of the poles, but is attributable to a higher temperature of the whole globe, whether due to external or internal causes, to the great amount of carbonic acid and water in the atmosphere, which would shut in and accumulate the sun's heat, according to the principles discovered by Tyndall and applied by Sterry Hunt in explanations of geological times. Le Conte, however, admits the possibility of the earth's having occupied a warmer position in stellar space, of its having exhibited a more uniform distribution of surface temperature, and a different distribution of land and water.*

Before Count Saporta's Essay had reached this country† another contribution to the subject of the origin of existing Floras had been communicated to our own Geographical Society, by Mr. Thiselton Dyer, in a Lecture on "Plant Distribution as a field for Geographical

* Professor Jos. Le Conte, in "Nature," October 24, 1878, p. 668.

† Count Saporta's Essay was presented to the International Congress of Geographical Science which met in Paris in 1875, and was not received by me till the autumn of 1878, though it bears the date 1877 on the title page.

Research." Mr. Thiselton Dyer's order of procedure is the reverse of Count Saporta's, and his method entirely different. He first gives a very clear outline of the distribution of the principal existing Floras of the continents and islands of the globe, their composition, and their relations to one another, and to those of previous geological epochs. He then discusses the views of botanists respecting their origin and distinctive characters, and availing himself of such of their hypotheses as he thinks tenable, correlates these with those of palæontologists, and arrives at the conclusion "That the northern hemisphere has always played the most important part in the evolution and distribution of new vegetable types, or in other words, that a greater number of plants has migrated from north to south than in the reverse direction, and that all the great assemblages of plants which we call Floras, seem to admit of being traced back at some time in their history to the northern hemisphere." This amount of accordance between the results of naturalists working wholly independently, from entirely different stand-points, and employing almost opposite methods, cannot but be considered as very satisfactory. I will conclude by observing that there is a certain analogy between two very salient points which are well brought out by these authors respectively. Count Saporta, looking to the past, makes it appear that the fact of the several Floras which have flourished on the globe being successively both more localised and more specialised, is the natural result of conditions to which it is assumed our globe has been successively subjected. Mr. Dyer, looking to the present, makes it appear that the several Floras now existing on the globe are, in point of affinity and specialisation, the natural results of the conditions to which they must have been subjected during recent geological time on continents and islands with the configuration of those of our globe.

The modern development of botanical science, being that which occupies my own attention, is naturally that on which I might feel especially inclined to dwell; and I should so far have the excuse that there is, perhaps, no branch of research with the early progress of which this Society is more intimately connected.

One of our earliest Secretaries, Robert Hooke, two centuries ago, laboured long and successfully in the improvement of the microscope as an implement of investigation. He was one of the first to reap the harvest of discovery in the new fields of knowledge to which it was the key, and if the results which he attained have rather the air of spoils gathered hither and thither in a treasury, the very fulness of which was embarrassing, we must remember that we date the starting point of modern histology from the account given by Hooke in his "Micrographia" (1667) of the structure of cork, which had attracted his interest from the singularity of its physical properties.

Hooke demonstrated its *cellular structure*, and by an interesting coincidence he was one of the first to investigate, at the request, indeed, of the founder of the Society, Charles II, the movements of the sensitive plant *Mimosa pudica*—one of a class of phenomena which is still occupying the attention of more than one of our Fellows. In attributing the loss of turgescence, which is the cause of the collapse of the petiole and subordinate portions of the compound leaf which it supports, to the escape of a subtle humour, he to some extent foreshadowed the modern view which attributes the collapse of the cells to the escape of water by some mechanism far from clearly understood—whether from the cell-cavities, or from the cell-walls into the intercellular spaces.

Hooke having shown the way, Nehemiah Grew, who was also Secretary of the Royal Society, and Marcello Malpighi, Professor of Medicine in the University of Bologna, were not slow to follow it. Almost simultaneously (1671–3) the researches of these two indefatigable students were presented to the Royal Society, and the publication of two editions of Malpighi's works in London proves how entirely this country was at that time regarded as the head quarters of this branch of scientific inquiry. We owe to them the generalisation of the cellular structure, which Hooke had ascertained in cork, for all other vegetable tissues. They described also accurately a host of microscopic structures then made known for the first time. Thus, to give one example, Grew figured and described in several different plants the stomata of the epidermis:—"Passports," as he writes, either "for the better avolation of superfluous sap, or the admission of air."

With the exception of Leeuwenhoek no observer attempted to make any substantial addition to the labours of Grew and Malpighi for more than a century and a half, and however remarkable is the impulse which he gave to morphological studies, the view of Caspar Wolff in the middle of the 18th century (1759), in regarding cells as the result of the action of an organizing power upon a matrix, and not as themselves influencing organization, were adverse to the progress of histology. It is from Schleiden (1838) who described the cell as the true unit of vegetable structure, and Schwann who extended this view to all organisms whether plants or animals, and gave its modern basis to biology by reasserting the unity of organization throughout animated nature, that we must date the modern achievements of histological science. Seldom, perhaps, in the history of science has any one man been allowed to see so magnificent a development of his ideas in the space of his own lifetime as has slowly grown up before the eyes of the venerable Schwann, and it was, therefore, with peculiar pleasure that a letter of congratulation was entrusted by your Officers to one of our Fellows on behalf of this Society on the recent occasion of the celebration of the 40th anniversary of Schwann's entry into the professorate.

If we call up in our mind's eye some vegetable organism and briefly reflect on its construction, we see that we may fix on three great steps in the analysis of its structure, the organic, the microscopic, and the molecular, and, although not in the same order, each of the three last centuries is identified with one of these. In the 17th century Grew achieved the microscopic analysis of plant tissues into their constituent cells; in the 18th, Caspar Wolff effected the organic analysis (independently but long subsequently expounded by the poet Goethe) of plant structures into stem and leaf. It remained for Nägeli in the present century to first lift the veil from the mysterious processes of plant growth, and by his memorable theory of the molecular constitution of the starch-grain and cell-wall, and their growth by intussusception (1858), to bring a large class of vital phenomena within the limits of physical interpretation. Strasburger has lately (1876) followed Sachs in extending Nägeli's views to the constitution of protoplasm itself, and there is now reason to believe that the ultimate structure of plants consists universally of solid molecules (not however identical with chemical molecules) surrounded with areas of water which may be extended or diminished. While the molecules of all the inert parts of plants (starch-grains, cell-wall, &c.) are on optical grounds believed by most physiologists to have a definite crystalline character, no such conclusion can be arrived at with respect to the molecules of protoplasm. In these molecules the characteristic properties of the protoplasm reside, and are more marked in the aggregate mass in proportion to its denseness, and this is due to the close approximation of the molecules and the tenuity of their watery envelopes. The more voluminous the envelopes, the more the properties of protoplasm merge in those of all other fluids.

It is, however, to the study of the nuclei of cells that attention has been recently paid with the most interesting results. These well-known structures, first observed by Ferdinand Bauer at the beginning of the century (1802), were only accurately described thirty years later by Robert Brown (1833). Up to the present time their function has been extremely obscure. The beautiful investigations of Strasburger (1875) have led him to the conclusion that the nucleus is the seat of a central force which has a kind of polarising influence upon the protoplasm molecules, causing them to arrange themselves in lines radiating outwards. Cell-division he regards as primarily caused by the nucleus becoming bipolar, and the so-called caryolitic figures first described by Auerbach, exhibit the same arrangement of the protoplasm molecules in connecting curves as in the case of iron-filings about the two poles of a bar-magnet. The two new centres mutually retire, and each influencing its own tract of protoplasm, the cell-division is thereby ultimately effected. This is but a brief account of processes which are greatly complicated in

actual detail, and of which it must be remarked that while the interest and beauty of the researches are beyond question, caution must be exercised in accepting the mechanical speculations by which Strasburger attempts to explain them. He has himself shown that cell-division presents the same phenomena in the animal kingdom; a result which has been confirmed by numerous observers, amongst whom I may content myself with mentioning one of our own Society, Mr. F. Balfour. Strasburger further points out that this affords an argument for the community of descent in animal and vegetable cells; he regards free cell-division as derivable from ordinary cell-division by the suppression of certain stages.

Turning now to the discoveries made during the last five years in Physiological Botany, we find that no one has advanced this subject so greatly as Mr. Darwin. In 1875 was published his work on Insectivorous Plants, in which he ascertained the fact that a number of species having elaborate structures adapted for the capture of insects, utilized the nitrogenous matter which these contain as food. The most important principle established in the course of these researches was, that such plants as *Drosera*, *Dionæa*, *Pinguicula*, secrete a digestive fluid, which has led through Gorup Bezanez's investigations on the ferment in germinating seeds, to a recognition of the active agency of ferments in the transmission of food-material, which marks a great advance in our knowledge of the general Physiology of Nutrition.

The extreme sensitiveness of the glands of *Drosera* to mechanical and chemical stimulus (especially to phosphate of ammonia), the directive power of its tentacles, depending upon the accurate transmission of motor impulses, and the "reflex" excitation of secretion in the glands, were all discoveries of the most suggestive nature in connexion with the subject of the irritability and movements of plants. The phenomenon of the aggregation of the protoplasmic cell-contents in the tentacles of *Drosera* is a discovery of a highly remarkable nature, though not yet thoroughly understood. Lastly, Mr. Frank Darwin, following his father's footsteps, as it were crowned the edifice by showing to what an extent insectivorous plants do profit by nitrogenous matter supplied to their leaves.

In close relation to these researches are those, also by Mr. Darwin, on the structure and functions of the bladder of *Utricularia*, which he has shown to have the power of absorbing decaying animal matter; and those of Mr. Frank Darwin on contractile filaments of extraordinary tenuity attached to the glands on the inner surface of the cups formed by the connate bases of the leaves of the Teasel, which filaments exhibit motions suggesting a protoplasmic origin. It is to be hoped that their discoverer will pursue his investigations into these curious bodies, whose origin and real nature in relation to the plant and its functions are involved in obscurity.

The subject of the cross-fertilization of plants, which though a long known phenomenon, first become a fruitful scientific study in Mr. Darwin's now classical work "On the various contrivances by which Orchids are fertilized," has within the last few years made rapid advance under its author's hand. The extreme importance of avoiding self-fertilization might indeed be inferred from the prevalence in flowers of elaborate contrivances for preventing it; but it remained to be shown that direct benefit attended cross-fertilization, and this has now been proved by an elaborate series of experiments, the results of which are not only that both increased fertility or greater vigour of constitution attend cross-fertilization, but that the opposite effects attend self-fertilization. In the course of these experiments it became evident that the good effects of the cross do not depend on the mere fact of the parents being different individuals, for when these were grown together and under the same conditions, no advantage was gained by the progeny; but when grown under different conditions a manifest advantage was gained. As instances, if plants of *Ipomœa* and *Mimulus*, which had been self-fertilized for seven previous generations, were kept together and then intercrossed, their offspring did not profit in the least; whereas, when the parent plants were grown under different conditions, a remarkably vigorous offspring was obtained.

Mr. Darwin's last work, "On the different forms of Flowers," though professedly a reprint of his paper on dimorphic plants, published by the Linnæan Society, contains many additions and new matter of great importance concerning the behaviour of polygamous plants, and on Cleistogamic flowers. Among other points of great interest is the establishment of very close analogies between the phenomena attending the illegitimate union of trimorphic plants, and the results of crosses between distinct species, the sterile offspring of the crosses of the same species exhibiting the closest resemblance to the sterile hybrids obtained by crossing distinct species; while a whole series of generalizations, founded on the results of the one series of experiments, are closely paralleled by those founded on the other. The bearing of this analogy on the origin of species is obviously important.

Besides these investigations, Mr. Darwin has produced within the last five years second editions of his volume on the Fertilization of Orchids, and on the Habits and Movements of Climbing Plants; as also of his early works on Coral Reefs, and Geological Observations in South America; all of them abounding in new matter.

Of special interest to myself, as having been conducted in the Jodrell Laboratory at Kew, are Dr. Burdon Sanderson's investigations on the exceptional property possessed by the leaves and other organs of some plants which exhibit definite movements in

response to mechanical, chemical, or electric stimuli. In 1873, Dr. Sanderson showed us in this meeting room, that the closing of the laminæ of the leaf of *Dionæa* is preceded by a preliminary state of excitement, and is attended with a change in the electric conditions of the leaf; and this so closely resembled the change which attends the excitation of the excitable tissues of animals, that he did not hesitate to identify the two phenomena.

This remarkable discovery immediately directed the attention of two German observers to the electromotive properties of plants, one, Dr. Kunkel, in the Laboratory of Professor Sachs; the other Professor Munk, in that of the University of Berlin.

Professor Munk, whose researches are of much the greater scope and importance, took as his point of departure Dr. Burdon Sanderson's discovery. The leading conclusion to which he arrived was, that in *Dionæa* each of the oblong cells of the parenchyma is endowed with electromotive properties, which correspond with those of the "muscle-cylinder" of animals; with this exception, that whereas in the muscle-cylinder the ends are negative to the central zone, in the vegetable cell they are positive; and he endeavours to prove, that according to this theory, all the complicated electromotive phenomena which had been observed, could be shown to be attributable to the peculiar arrangement of the leaf-cells.

During the last two summers Dr. Burdon Sanderson has been engaged in endeavouring to discover the true relations which subsist between the electrical disturbance, followed by the shutting of the leaf-valves of *Dionæa* and the latent change of protoplasm which precedes this operation. He has found that though the mechanism of the change of form of the excitable parenchyma which causes the contraction is entirely different from that of muscular contraction, yet that the correspondence between the exciting process in the animal tissues, and what represents this in plant tissues appears to be more complete the more carefully the comparison is made; and that whether the stimulus be mechanical, thermal, or electrical, its effects correspond in each case. Again, the excitation is propagated from the point of excitation to distant points in the order of their remoteness, and the degree to which the structure is excited depends upon its temperature. Notwithstanding, however, the striking analogies between the electrical properties of the cells of *Dionæa* and of muscle-cylinders, Dr. Burdon Sanderson is wholly unable to admit with Professor Munk that these structures are in this respect comparable.

In Morphological Botany attention has been especially directed of late to the complete life-history of the lower order of Cryptogams, since this is seen to be more and more an indispensable preliminary to any attempt at their correct classification.

The remarkable theory of Schwendener, now ten years old, astonished botanists by boldly sweeping away the claims to autonomous recognition of a whole group of highly characteristic organisms—the Lichens—and by affirming that these consist of ascomycetal fungi united in a commensal existence with Algæ. The controversial literature and renewed investigations which this theory has given rise to are now very considerable. But the advocates of the Schwendenerian view have gradually won their ground, and the success which has attended the experiments of Stahl in taking up the challenge of Schwendener's opponents and manufacturing such lichens as *Endocarpon* and *Thelidium*, by the juxtaposition of the appropriate Algæ and Fungi, may almost be regarded as deciding the question. Sachs, in the last edition of his Lehrbuch, has carried out completely the principle of classification of Algæ, first suggested by Cohn, and has proposed one for the remaining Thallophytes, which disregards their division into Fungi and Algæ. He looks upon the former as standing in the same relation to the latter as the so-called Saprophytes (*e.g. Neottia*) do to ordinary green flowering-plants.

This view has especial interest with regard to the minute organisms known as *Bacteria*, a knowledge of the life-history of which is of the greatest importance, having regard to the changes which they effect in all lifeless and, probably, in all living matter prone to decomposition. This affords a morphological argument (as far as it goes) against the doctrine of Spontaneous Generation, since it seems extremely probable that just as yeast may be a degraded form of some higher fungus, *Bacteria* may be degraded allies of the *Oscillatoriæ* which have adopted a purely saprophytal mode of existence.

Your "Proceedings" for the present year contain several important contributions to our knowledge of the lowest forms of life. The Rev. W. H. Dallinger, continuing those researches which his skill in using the highest microscopic powers and his ingenuity in devising experimental methods have rendered so fruitful, has adduced evidence which seems to leave no doubt that the spores or germs of the monad which he has described differ in a remarkable manner from the young or adult monads in their power of resisting heated fluids. The young and adult monads, in fact, were always killed by five minutes' exposure to a temperature of 142° F. (61° C.), while the spores germinated after being subjected to a temperature of ten degrees above the boiling point of water (222° F.).

Two years ago, Cohn and Koch observed the development of spores within the rods of *Bacillus subtilis* and *B. anthracis*. These observations have been confirmed, with important additions, in these two species by Mr. Ewart, and have been extended to the *Bacillus* of the infectious pneumo-enteritis of the pig, by Dr. Klein; and to *Spirillum* by Messrs. Geddes and Ewart; and thus a very important step has

been made towards the completion of our knowledge of the life-history of these minute but important organisms. Dr. Klein has shown that the infectious pneumo-enteritis, or typhoid fever of the pig, is, like splenic fever, due to a *Bacillus*. Having succeeded in cultivating this *Bacillus* in such a manner as to raise crops free from all other organisms, Dr. Klein inoculated healthy pigs with the fluid containing the *Bacilli*, and found that the disease in due time arose and followed its ordinary course. It is now therefore, distinctly proved that two diseases of the higher animals, namely, "splenic fever" and "infectious pneumo-enteritis," are generated by a *contagium vivum*.

Finally, Messrs. Downes and Blunt have commenced an enquiry into the influence of light upon *Bacteria* and other *Fungi*, which promises to yield results of great interest, the general tendency of these investigations leaning towards the conclusion that exposure to strong solar light checks and even arrests the development of such organisms.

The practical utility of investigations relating to *Bacillus* organisms as affording to the pathologist a valuable means of associating by community of origin various diseases of apparently different character, is exemplified in the "Loodiana fever," which has been so fatal to horses in the East. The dried blood of horses that had died of this disease in India has been recently sent to the Brown Institution, and from seeds therein contained a crop of *Bacillus anthracis* has been grown, which justified its distant pathological origin by reproducing the disease in other animals. Other equally interesting experiments have been made at the same Institution, showing that the "grains" which are so largely used as food for cattle, afford a soil which is peculiarly favourable for the development and growth of the spore filaments of *Bacillus*; and that by such "grains" when inspected, the anthrax fever can be produced at will, under conditions so simple that they must often arise accidentally. The bearing of this fact on a recent instance in which anthrax suddenly broke out in a previously uninfected district, destroying a large number of animals, all of which had been fed with grains obtained from a particular brewery, need scarcely be indicated.

In Systematic Botany, which in a nation like ours, ever extending its dominions and exploring unknown regions of the globe, must always absorb a large share of the energies of its phytologists, I can but allude to two works of great magnitude and importance.

Of these, the first is the "Flora Australiensis" of Bentham, completed only a year ago; a work which has well been called unique in botanical literature, whether for the vast area whose vegetation it embraces (the largest hitherto successfully dealt with), or for the masterly manner in which the details of the structure and affinities of upwards of 8,000 species have been elaborated. Its value in reference

to all future researches regarding the geographical distribution of plants in the southern hemisphere, and the evolution therein of generic and specific types, cannot be over estimated.

The other great work is the "Flora Braziliensis," commenced by our late Foreign Fellow, von Martius, and now ably carried on by Eichler, of Berlin, assisted by coadjutors (among whom are most of our leading systematists) under the liberal auspices of His Majesty the Emperor of Brazil. When completed, this gigantic undertaking will have embraced, in a systematic form, the vegetation of the richest botanical region of the globe.

Having now endeavoured to recall to you some of the great advances in Science made during the last few years, it remains for me, after the distribution of the Medals awarded by your Council, to retire from the Presidency in which I have so long experienced the generous support of your Officers and yourselves. This support, for which I tender you my hearty thanks, together with my sense of the trust and dignity of the office, and the interest attached to its duties, make my resignation of it a more difficult step than I had anticipated. My reasons are, however, strong. They are the pressure of official duties at Kew, annually increasing in amount and responsibility, together with the engagements I am under to complete scientific works, undertaken jointly with other botanists, before you raised me to the Presidency; and the fact that indefinite postponement delays the publication of the labours of my coadjutors. I am also influenced by the consideration that, though wholly opposed to the view that the term of the Presidency of the Royal Society should be either short or definitely limited, this term should not be very long; and that, considering the special nature of my own scientific studies, it should, in my case, on this as well as on other grounds, be briefer than might otherwise be desirable. Cogent as these reasons are, they might not have been paramount, were it not that we have among us, one pre-eminently fitted to be your President by scientific attainments, by personal qualifications, and by intimate knowledge of the Society's affairs; and by calling upon whom to fill the proud position which I have occupied, you are also recognising the great services he has rendered to the Society as its Treasurer for eight years, and its ofttimes munificent benefactor.

On the motion of Dr. Graham Balfour, seconded by Sir Alexander Armstrong, it was resolved—"That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed."

The President then proceeded to the presentation of the Medals.

The Copley Medal has been awarded to Jean Baptiste Boussingault

for his long-continued and important researches and discoveries in agricultural chemistry.

The researches of Boussingault have extended over nearly half a century, and it might be difficult to find an investigator whose results relating to a great variety of subjects have in respect of accuracy and trustworthiness better stood the test of time.

The lucid simplicity with which his writings narrate well-established and well-arranged facts, is not less remarkable than the judicial caution with which he has abstained from expressing opinions upon questions beyond the reach of decisive evidence.

His experimental results and the conclusions which he has drawn from them have been deservedly trusted by other workers in the same field, and have safely guided them in their labours. Their incontestable excellence has prevented them from becoming subjects of animated discussion, and thus arousing as much attention and interest in the outer world as has sometimes been aroused by hasty experiments and daring generalizations.

I cannot attempt within the limits of this address to give an account of his investigations, and I should probably weary you were I even to enumerate them, relating as they do to a vast variety of phenomena; but I may point out that lying as most of them do in the domain of agricultural chemistry, they have involved difficulties of no common order. Boussingault is not only an excellent chemical analyst and experimentalist, but at the same time a model farmer.

His numerous determinations of the nitrogen, carbon, and hydrogen in crops and in the manures supplied to them, have proved him to be skilled not only in selecting and applying the best known methods of analysis, but even in improving and perfecting them.

His determinations of the proportions of those valuable constituents of manures which can be assimilated by various crops, have involved an intimate acquaintance with the conditions which experience has proved to be most favourable to the cultivation of the various crops.

His numerous and varied experiments on the feeding of animals, showing the proportions between the nitrogenized and fatty or amylaceous constituents supplied in the food and those assimilated or formed by the animal organism, while tracing the distribution of the remainder between the pulmonary and other excretions, have had most important physiological as well as practical bearings.

In all his investigations we see proofs that while accurately and critically acquainted with the discoveries and opinions of other workers and thinkers in his own particular domain of science, he has been able to devise and carry out simple and crucial forms of experiment well calculated to decide the truth.

A remarkable instance of this is afforded by those truly masterly experiments by which he proved that all the nitrogen found in the

organism of plants can be traced to compounds of that element which had been supplied to them; and accordingly that there are no grounds for believing that plants can assimilate the free nitrogen of the air.

By awarding to Boussingault the Copley Medal, we place his name in the honoured list of those who, in modern times, have rendered the highest services to the advancement of natural knowledge.

A Royal Medal has been awarded to Mr. John Allan Broun for his investigations during thirty-five years in magnetism and meteorology, and for his improved methods of observation.

When the labours of Gauss had given an impetus to the study of terrestrial magnetism by rendering precision possible, Observatories devoted to this branch of research, in conjunction with meteorology, began to rise in various places. The late General Sir T. M. Brisbane erected one at Makerstown, in Scotland, and placed it under the direction of Mr. Broun, who remained in charge of it from 1842 to 1850. His observations and their results, have been commended by magneticians and meteorologists, for the skill employed in the development of new methods of reduction and investigation.

In 1851 Mr. Broun went to India to organize and take charge of a similar Observatory established at Trevandrum by His Highness the late Rajah of Travancore. Here he remained for thirteen years, accumulating results of very great value, the first instalment of which, consisting of a volume on the magnetic declination, was published some years ago. Magneticians look eagerly towards the completion of this publication when the means necessary for the purpose shall have been furnished to Mr. Broun.

While in India he established an Observatory on a mountain peak 6,000 feet above the sea, and fitted it up with a very complete assortment of scientific instruments. This was an undertaking of a very arduous nature, effected in a wild country, and presenting great difficulties in the erection of instruments and obtaining trained observers.

Shortly after the commencement of magnetic observatories, Mr. Broun indicated the insufficiencies of the methods then in use for determining coefficients and correcting observations, and he devised new methods for these ends, the principal of which have been generally adopted.

This is not the place in which to give a complete catalogue of Mr. Broun's researches in magnetism and meteorology, extending as they do over a period of thirty-five years, but I may indicate those of his results that are of the greatest importance. Among them are the establishment of the annual laws of magnetic horizontal force, exhibiting maxima at the solstices and minima at the equinoxes. Mr. Broun was also the first to give in a complete form the laws of change of the solar-diurnal variation of magnetic declination near the

equator, showing the extinction of the mean movement near the equinox. His researches on the lunar-diurnal variation of magnetic declination are of very great interest. Besides being an independent discoverer of the existence of this variation, he showed that near the equator its law in December was the opposite of that in June. He found, too, that the lunar-diurnal variation was in December sometimes greater than the solar-diurnal variation—that the lunar action was reversed at sunrise, and that it was much greater during the day than during the night, whether the moon was above or below the horizon. Finally, he found that the lunar-diurnal law changed (like the solar-diurnal law at the equator) near the equinoxes, so that, as a consequence, the laws for the southern and northern hemispheres were of opposite natures.

Another and very remarkable fact discovered by Mr. Broun was that the variations from day to day of the earth's daily mean horizontal force were nearly the same all the world over. He found certain oscillations in these daily means which were due to the moon's revolution, and others having a period of twenty-six days; the latter he considered as due to the sun's rotation. It results from these investigations that the observed variations of the earth's daily mean horizontal force have been represented with considerable accuracy in all their more marked features, by the combination of the means calculated for these different solar and lunar periods. During the discussion of these periods, Mr. Broun found that the great magnetic disturbances were apparently due to actions proceeding from particular points or meridians of the sun—a fact this (if verified) of very great importance.

In meteorology he has shown the apparent simultaneity of the changes of daily mean barometric pressure over a great part of the globe, and he has likewise discovered a barometric period of twenty-six days nearly. He was also the first to commence and carry out, during several years, a systematic series of observations of the motions of clouds at different heights in the atmosphere; and, lastly, he has found certain laws connecting the motions of the atmosphere, and the directions of the lines of equal barometric pressure.

A Royal Medal has been awarded to Dr. Albert Günther, F.R.S., for his numerous and valuable contributions to the zoology and anatomy of fishes and reptiles.

Dr. Günther's labours as a systematist and a descriptive zoologist have been devoted chiefly to the order of Fishes, Reptiles, and Amphibia. Upon these he has published during the last quarter of a century a very long series of valuable papers, whereby our knowledge of the structure, affinities, and distribution of the genera and species of those interesting groups has been greatly advanced. We owe to his indefatigable exertions the excellent condition in point of arrangement

and nomenclature of the unrivalled collection of fishes in the British Museum, and of which he prepared a systematic catalogue in eight volumes, which has been published by order of the Trustees. This is a work of prodigious labour; it required for its satisfactory execution an intimate knowledge of the fish of all parts of the world, and an intuitive perception of the natural character upon which a sound classification should be based. From possessing these attributes it has been accepted as the standard authority on the order by all zoologists. Under this head too I must specially allude to his excellent work on the Ceratodus. The Reptilian collections of the Museum have been no less successfully dealt with by Dr. Günther, and have afforded the material for some of his most important works, amongst which his "Reptiles of British India," "Memoir on Hatteria," and "Monograph of the Gigantic Land Tortoises of certain islands in the Pacific and Indian Oceans," are examples conspicuous for their completeness and accuracy.

The Rumford Medal has been awarded to Mr. Alfred Cornu for his various Optical Researches, and especially for his recent redetermination of the Velocity of Propagation of Light.

Mr. Alfred Cornu is the author of papers on optical and other subjects published in the "Comptes Rendus" and other scientific periodicals. He has been engaged, for example, with the difficult subject of crystalline reflection, and kindred researches.

It was in 1849 that Fizeau astonished the scientific world by an actual experimental determination of the velocity of light, a velocity so enormous that hitherto its finiteness has been proved, and its value approximately determined, only by two astronomical phenomena. Foucault almost simultaneously brought out an experimental determination by a totally different method.

The method of Fizeau gave at once a near approximation to the value got from those two astronomical phenomena, combined with the parallax of the sun, assumed known. But the difficulties of obtaining a sufficiently accurate result were such that the velocity obtained by Fizeau's method was not considered to rival in exactness the velocity determined astronomically. Indeed, Foucault's method seemed to be preferred, though Fizeau's is the simpler in principle, and is free from certain doubts which may be raised as regards the other. But the difficulties alluded to, which turned mainly on the determination of the velocity of the revolving wheel, were such that almost twenty years have elapsed without the method having been brought to a sufficient degree of perfection to make it astronomically available.

Such was the state of the problem when it was taken up by M. Cornu. By methods of his own devising he succeeded in getting over the difficulties with which Fizeau's further progress had been

stopped, and in achieving a determination so exact as to compete with the astronomical determinations, and thereby lead, we may say, to an experimental determination of the solar parallax fully rivalling that which is likely to result from the observations of the transit of Venus which have been carried out at so much cost and trouble.

A double award of the recently instituted Davy Medal has again been made, the recipients on the present occasion being M. Louis Paul Cailletet and M. Raoul Pictet. This award is made to these distinguished men for having, independently and contemporaneously, liquefied the whole of the gases hitherto called permanent.

The methods pursued by these experimenters, in accomplishing results which equal in interest and importance those obtained by Faraday in the same direction fifty-five years ago, were quite distinct, and were, in each case, the result of several years' preparatory labour. M. Cailletet, by comparatively very simple arrangements, such as admit of ready employment in lecture-demonstrations, has succeeded in obtaining evidence of the liquefaction, and possibly solidification, of carbonic oxide, marsh-gas, oxygen, nitrogen, and hydrogen. His system of operating consists in submitting the gases to very powerful compression at comparatively moderate degrees of cold, and in then allowing them very suddenly to expand.

M. Pictet has applied the very perfect system, elaborated and put to industrial use by him, for obtaining low temperatures to the attainment, though on a larger scale, of results like some of those arrived at by M. Cailletet. By an arrangement of vacuum and force pumps he reduces liquefied sulphurous acid to a low temperature, and applies this as the means for cooling down liquid carbonic acid which, in turn, serves to reduce to a very low temperature a thick glass tube, in which the gas to be condensed is confined at a very high pressure. M. Pictet has not only produced liquid oxygen in somewhat considerable quantity, and succeeded in determining its density, he has also obtained evidence of the solidification of hydrogen, and the description given of its appearance in the solid form seems to leave no doubt regarding its metallic character.

The interest which attaches to the remarkable experiments of MM. Cailletet and Pictet is only equalled by the importance of the fact, now absolutely demonstrated by those experiments, that the property of molecular cohesion is common to all known bodies without exception.

The Statutes relating to the election of Council and Officers were then read, and Mr. Ellis and Mr. McLachlan having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows

present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—William Spottiswoode. M.A., D.C.L., LL.D.

Treasurer.—John Evans, F.G.S., F.S.A.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Thomas Henry Huxley, LL.D.

Foreign Secretary.—Alexander William Williamson, Ph.D.

Other Members of the Council.—Frederick A. Abel, C.B., V.P.C.S.; William Bowman, F.R.C.S.; William Carruthers, V.P.L.S.; Major-General Henry Clerk, R.A.; William Crookes, V.P.C.S.; Sir William Robert Grove, M.A.; Augustus G. Vernon Harcourt, F.C.S.; Sir Joseph Dalton Hooker, C.B., K.C.S.I., D.C.L.; Admiral Sir Astley Cooper Key, K.C.B.; Lieut.-General Sir Henry Lefroy, C.B.; Lord Lindsay, P.R.A.S.; Sir John Lubbock, Bart., V.P.L.S.; Lord Rayleigh, M.A.; Charles William Siemens, D.C.L.; John Simon, C.B., D.C.L.; Professor Allen Thomson, M.D., F.R.S.E.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	Total.
November 30, 1877.	4	43	252	253	552
Elected		+ 7	+ 4	+ 13	+ 24
Deceased		− 6	− 5	− 16	− 27
Since compounded			+ 2	− 2	
November 30, 1878.	4	44	253	248	549

Statement of Receipts and Expenditure from November 23, 1877, tc November 28, 1878.

Receipts	£	s.	d.
Annual Contributions	1,032	0	0
Admission Fees	170	0	0
Compositions	312	0	0
Sale £15,727 10*s.* 6*d.* Consols	15,000	0	0
Rents	321	4	8
Dividends (exclusive of Trust Funds)	1,345	17	11
„ on Jodrell Fund	183	10	9
Interest on Mortgage Loan	294	9	8
Sale of Transactions, Proceedings, &c.	716	16	6
Sundries	2	6	0
Donations for cost of Plates	100	0	0
„ for Donation Fund	5	5	0
Balances from 1877	933	11	1
	£20,417	1	7

Expenditure	£	s.	d.	£	s.	d.
Mortgage Loan				15,000	0	0
Bought £662 16*s.* Consols				628	0	0
Salaries and Wages				1,120	4	0
Illustrations and Paper for Report of Naturalists (Transit-of-Venus Expedition)				199	10	5
The Scientific Catalogue				210	12	0
Books for the Library				142	11	11
Binding ditto				100	17	4
Printing Transactions, Part II. 1877, and Part I. 1878, and Separate Copies to Authors and Publisher	388	19	10			
Ditto Proceedings, Nos. 170–182	395	8	10			
Ditto Miscellaneous	67	4	1	1,903	6	10
Paper for Transactions and Proceedings	418	15	0			
Binding and Stitching ditto	114	16	11			
Engraving and Lithography	518	2	2			
Soirée and Reception Expenses				90	12	3
Coal, Lighting, &c.	90	14	5			
Office Expenses	29	4	9			
House Expenses	91	1	7			
Tea Expenses	20	8	6			
Fire Insurance	41	15	0	351	6	9
Taxes	22	13	9			
Advertising	16	19	0			
Postage, Parcels, and Petty Charges	36	7	9			
Mablethorpe Schools, Donation	2	2	0			
				£19,747	1	6

Trust Funds.

	£ s. d.	£ s. d.
Donation Fund Dividends	186 18 5	
Handley Fund "	173 19 11	
Rumford Fund "	68 10 7	
Wintringham Fund "	35 8 0	507 4 2
Copley Medal Fund "	9 18 4	
Davy Medal Fund "	32 8 11	
		£20,924 5 9

	£ s. d.	£ s. d.
Donation Fund	105 5 0	
Davy Medal Fund	108 5 1	
Wintringham Fund	35 11 0	
Copley Medal Fund	4 15 1	260 19 8
Bakerian Lecture	4 4 3	
Croonian Lecture	2 19 3	
Balance at Bank		894 2 3
Balances on hand, Catalogue and Petty Cash		22 2 4
		£20,924 5 9

W. SPOTTISWOODE,
Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £136 per annum.
Estate at Acton, Middlesex (34 A. 2 R. 4¾ P.), £167 17*s*. 10*d*. per annum.
Fee Farm near Lewes, Sussex, rent £19 4*s*. per annum.
One fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
Stevenson Bequest. Chancery Dividend. One fourth annual interest on £85,336, Government Annuities and Bank Stock (produced £485 3*s*. 4*d*. in 1877–78).
£19,898 2*s*. 5*d*. Reduced 3 per Cent. Annuities.
£15,000 Mortgage Loan, 4 per Cent.
£16,588 6*s*. 2*d*. Consolidated Bank Annuities.
£403 9*s*. 8*d*. New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.
£6,221 14*s*. 1*d*. New Threes.—Jodrell Fund.
£667 5*s*. 6*d*. India Fours.
£660 Madras Guaranteed 5 per Cent. Railway Stock.—Davy Medal Fund.
£10,000 Italian Irrigation Bonds.—The Gassiot Trust.

Trust Funds. 1878.

Scientific Relief Fund.

	£	s.	d.
New 3 per Cent. Annuities	6,328	11	2
Metropolitan 3½ Consols	100	0	0
	£6,428	11	2

Dr. / *Cr.*

Dr.	£	s.	d.	Cr.	£	s.	d.
Balance	229	12	9	By Grants	250	0	0
Dividends	190	3	10	Expenses	0	5	0
				Balance	169	11	7
	£419	16	7		£419	16	7

Donation Fund.

£6,339 0*s.* 1*d.* Consols.

Dr.	£	s.	d.	Cr.	£	s.	d.
To Balance	246	2	7	By Grants	300	0	0
Dividends	186	18	5	Bought £5 9*s.* 9*d.* Consols	5	5	0
Donation	5	5	0	Balance	133	1	0
	£438	6	0		£438	6	0

Rumford Fund.

£2,322 10s. Consols.

	£	s.	d.		£	s.	d.
Balance, 1877	68	16	6	Balance	137	7	1
Dividend, 1878	68	10	7				
	£137	7	1				

Bakerian and Copley Medal Fund.

£403 9s. 8d. New 2½ per Cent.

	£	s.	d.		£	s.	d.
To Balance	73	0	1	By Gold Medal	4	15	1
Dividends	9	18	4	Bakerian Lecture and Expenses	4	4	3
				Balance	73	19	1
	£82	18	5		£82	18	5

Wintringham Fund.

£1,200 Consols.

	£	s.	d.		£	s.	d.
To Balance, 1877	35	11	0	By Payment to Foundling Hospital, 1878	35	11	0
Dividends, 1878	35	8	0	Balance	35	8	0
	£70	19	0		£70	19	0

Croonian Lecture Fund.

	£	s.	d.		£	s.	d.
To one-fifth of Rent of Estate at Lambeth Hill, payable by the College of Physicians	2	19	3	By Croonian Lecture	2	19	3

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£ s. d.		£ s. d.
To Balance	246 3 5	By Dies and Fittings	48 4 1
Dividends	32 8 11	" Gold Medals	60 1 0
		Balance	170 7 3
	£278 12 4		£278 12 4

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

	£ s. d.		£ s. d.
To Balance	236 0 0	By Payments to Kew Committee	495 15 3
Dividends	495 15 3	Bonds bought	179 10 0
Bonds drawn	234 10 0	Balance	291 0 0
	£966 5 3		£966 5 3

The Jodrell Fund.

£6,221 14*s.* 1*d.* New 3 per Cent. Stock.

	£ s. d.		£ s. d.
Dividends, 1878	183 10 9	By Payment to Royal Society	183 10 9

The Handley Fund.

£5,898 2*s.* 5*d.* Reduced 3 per Cent. Stock.

	£ s. d.		£ s. d.	£ s. d.
Balance, 1877	405 18 11	By Payment to Royal Society, 1877	264 17 9	438 17 8
Dividends, 1878	173 19 11	" " " 1878	173 19 11	
		Balance of Capital Account		141 1 2
	£579 18 10			£579 18 10

Account of Grants from the Donation Fund in 1877–78.

Illustrations for the Naturalists' (Transit of Venus) Reports	£200	0	0
vans, for Exploration of Caves in Borneo	50	0	0
. H. Gordon, for continuation of Researches on the Specific Inductive Capacity of Dielectrics	25	0	0
R. Hodgkinson, for an Investigation on the Action of Ethylbenzol Acetate upon acetic, butyric, and isobutyric Ethers	25	0	0
	£300	0	0

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ount of the appropriation of the sum of £1,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the advancement of Science (continued from Vol. XXVI, p. 457).

1878.

W. Ramsay, for Apparatus to be employed in a Research the Action of Light and Heat in Decomposing Hydriodic Hydrobromic Acids, with a view to compare the action of ,t and Light	£30
. J. H. Poynting, to determine the Mean Density of the th by means of an Ordinary Balance in place of the Torsion ance, and to Investigate means to very greatly increase the uracy of the Balance	150
. Captain Abney, for registering the Intensity of the Spec- n of Daylight	50
. Dr. Duncan and P. Sladen, for Publication of a Monograph he Arctic Echinodermata, especially those of Smith's Sound to the North	60
. Bevan Lewis, for a Research into the Histology of the ebral Cortex in Man and the higher Mammalia, with especial rence to the Motor Area as defined by Professor Ferrier	15
. D. Mackintosh, for aid in tracking Streams of Erratic cks from their Parent Rocks, to ascertain the Character of Drift Deposits with which they are associated	25
. W. C. Williamson, for continuation of Researches into the sil Plants of the Coal Measures	30
Carried forward	£360

Brought forward	£360
8. Professor Lankester, for the Investigation of the Life-History and Specific Forms of Bacteria; the Relation of Special Forms to Special Putrefactive and other Physiological Activities; and the Generic and Specific Distinctions	40
9. A. Wynter Blyth, for continuation of Researches into the Chemical Constitution of the Poison of the Cobra de Capello ..	20
10. M. M. Pattison Muir, for Investigation of the Chemical Nature of Essential Oil of Sage, and the Determination of the Chemical and Physical Constituents of this Oil	30
11. A. Macfarlane, for Apparatus to continue and extend an exact experimental Research into the Conditions of Passage of the Disruptive Discharge of Electricity	50
12. W. Crookes, for continuing his Researches connected with Repulsion resulting from Radiation	200
13. Professor Church, for continuation of Researches in Plant Chemistry	50
14. E. Neison, for Computations in the Lunar Theory	75
15. G. J. Stoney, for completing a Spectroscope of great Aperture and continuing his Experiments on the Motions in Gases	100
16. B. Stewart, for analysing the Records of Magnetic Declination	75
17. Baron Ettingshausen, for travelling expenses, and maintenance in England during the preparation of a Monograph on the Eocene Flora of Great Britain	50
	£1050

Dr.		£	s.	d.	*Cr.*	£	s.	d.
To Balance on hand, Nov. 30, 1877		1015	13	6	By Appropriations, as above	1050	0	0
Grant from Treasury, 1878		1000	0	0	Printing, Postage, and Advertising	8	5	7
Repayments.					Balance on hand, Nov. 30, 1878	1032	5	8
R. H. Scott, balance	£35 0 0							
W. Ramsay ,,	30 0 0							
		65	0	0				
Interest		9	17	9				
		2090	11	3		2090	11	3

Account of appropriations from the Government Fund of £4,000 made by the Lords of the Committee of Council on Education, on the recommendation of the Council of the Royal Society.

1878.

D. Gill, to defray expenses connected with a Determination of the Solar Parallax by Observation of the Diurnal Parallax of Mars £250

Rev. Dr. Haughton, for aid in the Numerical Reductions of the Tidal Observations made on board the "Discovery" and "Alert" in the late Arctic Expedition..................... 75

J. A. Broun, for continuation of Correction of the Errors in the Published Observations of the Colonial Magnetic Observatories 150

J. P. Joule, for an Exhaustive Enquiry into the Change which takes place in the Freezing and Boiling Points of Mercurial Thermometers by Long Exposure to those Temperatures 200

Professor Jenkin, for Experimental Investigations on Friction 50

W. C. Roberts, for Researches on Metals and Alloys in a Molten State passing through Capillary Tubes 25

J. Kerr, for continuation of Electro-optic and Magneto-optic Researches 50

J. N. Lockyer, for continuation of Spectroscopic Researches.. 200

Dr. O. J. Lodge, for Investigations into the Effect of Light on the Residual Charge of Dielectrics; on the Conductivity of Hot Glass, and other Transparent Conductors; on Electrolytic Conduction and other subjects 100

Mr. Stevenson, for aid in carrying on Observations of the Temperature of Salmon Rivers in Scotland, and other Meteorological Observations 50

W. Galloway, for further Investigation of the Explosive Properties of Mixtures of Firedamp and Coal Dust with Air 100

Sir W. Thomson, for continuation of Tidal Investigation 100

Sir W. Thomson, for experiments in Magnetisation of Different Qualities of Iron, Nickel, and Cobalt under varying Stresses and Temperatures.. 100

J. E. H. Gordon, for continuation of Experimental Measurements of the Specific Inductive Capacity of Dielectrics 100

H. Tomlinson, for Researches on the Alteration of Thermal and Electrical Conductivity produced by Magnetism; and on the

Carried forward............. £1550

Brought forward.............	£1550
Alteration of Electrical Resistance produced in Wires by Stretching	100
H. Alleyne Nicholson and R. Etheridge, jun., for aid in examining the Fauna of the Silurian Deposits of the Girvan District, Ayrshire, and in publishing a Descriptive List of the same	75
W. K. Parker, for assistance in continuation of Researches on the Morphology of the Vertebrate Skeleton, and the Relations of the Nervous to the Skeletal Structure, chiefly in the Head ..	300
R. McLachlan, for aid towards the expense of publication of a Revision and Synopsis of European Trichoptera	50
C. Callaway, for aid in working out the so-called Eruptive Rocks of Shropshire, and in verifying certain points in Local Geology	25
H. T. Stainton, in aid of the Publication Fund of the Zoological Record Association	150
J. W. Dawson, for aid in excavating erect Trees in the Coal Formation of Nova Scotia, in Beds where they are known to contain Reptilian and other Remains.......................	50
Professor A. H. Garrod, for aid towards production of the Second Fasciculus of an exhaustive Treatise on the Anatomy of Birds	100
Rev. J. F. Blake, for aid in continuing the publication of a Synopsis of British Fossil Cephalopoda	100
Dr. W. A. Brailey, for Researches on the Causes determining the Tension of the Globe of the Eye in Man and Animals, and on the Physiological Influence on this Tension of such substances as Atropia, Daturin, Eserin, and Pilocarpine	50
W. Saville Kent, to pay for Microscopical Apparatus for the further Prosecution of Investigations into the Structure and Life-History of certain Lower Protozoa	50
Dr. R. H. Traquair, for aid in preparing and publishing a Monograph on the Carboniferous Ganoid Fishes of Great Britain	75
E. A. Schäfer, for payment of an Assistant in continuing his Histological and Embryological Investigations	50
H. Woodward, for continuation of work on the Fossil Crustacea, especially with reference to the Trilobita and other Extinct Forms, and their publication in the Volumes of the Palæontographical Society	75
Professor Seeley, for an Examination of the Structure, Affinities, and Classification of the extinct Reptilia and allied Animals..	75
Dr. Wright, for continuation of Researches on certain points	
Carried forward.............	£2875

Brought forward.............	£2875
in Chemical Dynamics; on the determination of Chemical Affinity in terms of Electrical Magnitudes; and on some of the less known Alkaloids	300
C. Schorlemmer, for continuation of Researches into (1) the Normal Paraffins (2) Suberone (3) Aurin	250
E. J. Mills, for a Research on Standard Industrial Curves ..	100
W. N. Hartley, for Investigation of the Fluid Contents of Mineral Cavities; of the Properties of the Phosphate of Cerium; of Methods of Estimating the Carbonic Acid in small samples of Air; and of Photographic Spectra...........................	150
Dr. Armstrong, for continuation of Researches into the Phenol Series	250
	3925
Administrative Expenses.............................	75
	£4000

Report of the Kew Committee for the Year ending October 31, 1878.

The Kew Committee has had its strength increased during the past year by the accession of two new members, Professor W. G. Adams and Professor G. C. Foster, and is now constituted as follows:—

General Sir E. Sabine, K.C.B., *Chairman.*

Mr. De La Rue, *Vice-Chairman.*	The Earl of Rosse.
Prof. W. G. Adams.	Mr. R. H. Scott.
Capt. Evans, C.B.	Lieut.-General W. J. Smythe.
Prof. G. C. Foster.	Lieut.-General R. Strachey, C.S.I.
Mr. F. Galton.	Mr. E. Walker.
Vice-Adm. Sir G. H. Richards, K.C.B.	

Magnetic Work.—The Magnetographs have been in constant operation throughout the year, but only few magnetic disturbances have been registered, the period being one of almost continued magnetic calm. The most notable perturbation was that of May 15th.

The scale values of all the instruments were re-determined in January, in accordance with the usual practice.

A slight alteration has been made in the cases enclosing the horizontal and vertical force magnets; zinc cylinders with glass covers being substituted for the glass shades lined with gold-leaf previously employed, which were found to be very expensive to replace in case of breakage.

The tabulation of the magnetic curves has not been continued during the year, the time of the department being very fully occupied with the verification of magnetic instruments.

The Committee have referred the whole subject of the reduction of the accumulated magnetograph records to a Sub-Committee, with a view of considering what steps shall be taken to utilize them to the best advantage.

The monthly observations with the absolute instruments have been

made as usual by Mr. Figg, and the results are given in Tables appended to this Report.

The catalogue of the documents and papers in the late Magnetic Office, directed by Sir E. Sabine, having been completed, a selection was made of all those relating to marine observations, and at the request of the Hydrographer, these were transferred to the Hydrographic Department of the Admiralty.

The magnetic instruments have been examined and knowledge of their manipulation obtained by Lieutenants Speelman and van Hasselt, of the Dutch Navy; Professor Greene, of the United States Navy; M. Hooreman, of the Brussels Observatory; and Dr. T. E. Thorpe, F.R.S.

The latter gentleman made a series of base observations at Kew before and after an extended tour, for the purpose of a magnetic survey along the fortieth parallel of latitude in the United States.

A large magnet and a journeyman clock, the property of the Royal Observatory, Greenwich, which have been for many years at Kew, have been returned to the former establishment at the request of the Astronomer Royal.

Information on matters relating to terrestrial magnetism and various data have been supplied to the Hydrographic Office, Mr. Adie, Mr. Archbutt, Mr. Gordon, and Mr. Frost.

Meteorological Work.—The several self-recording instruments for the continuous registration respectively of pressure, temperature, humidity, wind (direction and velocity), and rain have been maintained in regular operation under the care of Mr. T. W. Baker, assisted by J. Hillier.

The daily standard eye observations, for the control of the automatic records have been made regularly, as well as daily observations in connexion with the Washington synchronous system.

The tabulation of the meteorological traces has been regularly carried on by Mr. Hawkesworth, and copies have been transmitted weekly to the Meteorological Office.

In compliance with a request made by the Meteorological Council to the Kew Committee, the Observatories at Aberdeen, Armagh, Falmouth, Glasgow, Stonyhurst, and Valencia have been visited and their instruments inspected by Mr. Whipple, who has also inspected the telegraph-reporting and climatological stations throughout Ireland, an allowance has been made by the Meteorological Office to Kew, for the time occupied by Mr. Whipple on this duty.

With the sanction of the Meteorological Council, weekly abstracts of the meteorological results have been regularly forwarded to and published by "the Times," "Illustrated London News," and "Mid-Surrey Times;" and meteorological data have been supplied amongst others to Mr. G. J. Symons, F.R.S., Dr. Rowland, Mr. Mawley, and the Institute of Mining Engineers.

Electrograph.—This instrument has been in almost continuous action through the year under the care of Mr. Harrison. Certain improvements in minor details, suggested by Sir W. Thomson, have been introduced from time to time.

It has been thought desirable to make a determination of the scale value of the instrument throughout the whole extent of its range. The Committee not possessing a sufficiently powerful battery for the purpose, the Electrometer was conveyed at Mr. De La Rue's suggestion to his Laboratory, where a complete determination of its scale value was made over the range of tension afforded by 1,200 chloride of silver cells. A detailed account of the experiment was afterwards laid before the Royal Society, and printed in the "Proceedings," vol. xxvii, p. 356.*

At the request of Professor Mascart, a typical set of curves, illustrating the action of the Electrograph during different kinds of weather, was reduced and engraved by the Pantagraph at the Meteorological Office, and forwarded for his use in illustration of the lectures he delivered before the Société Météorologique de France.

These engravings have since been reproduced, together with notes respecting the instrument, in a Report on Atmospheric Electricity, drawn up by Professor Everett, for the Permanent Committee of the Vienna Congress, which is about to be published by the authority of the Meteorological Council.

The late Captain R. G. Scott, R.E., and since his decease, Captain R. Y. Armstrong, R.E., visited the Observatory and inspected the working of the instrument with the view of possibly utilizing the Electrometer in the study of atmospheric electricity at the various torpedo stations round the coast.

Two Electrographs, similar in construction to the instrument at Kew, have been constructed by Mr. White, of Glasgow, and after examination at the Observatory, forwarded, the one to the Brussels Observatory, the other to Zi-ka-Wei, China.

Photoheliograph.—The re-examination of the measurements of the Kew sun-pictures, as noticed in former Reports, has been steadily carried on throughout the year by Mr. Whipple, assisted by Mr. M'Laughlin, who has been temporarily engaged for this purpose.

During the year upwards of 400 pictures have been measured, and it is hoped that the end of the series will be reached in the early months of 1879.

* With the view of rendering the indications of the instrument better adapted for treatment with the Harmonic Analyser, it is in contemplation to somewhat alter the existing bifilar suspension of the needle, and at the same time to adopt the new insulating stand devised by Professor Mascart ("Nature," vol. xviii, p. 44) which will be substituted for the present supports of the water reservoir. These changes may cause a short discontinuity in the observations.

Mr. Marth is still engaged on the reduction to heliocentric elements of the pictures for 1864 to 1868 inclusive.

All of these operations have been conducted under the direction and at the expense of Mr. De La Rue.

The eye-observations of the sun, after the method of Hofrath Schwabe, have been made daily, when possible, as described in the Report for 1872, in order for the present to maintain the continuity of the Kew record of sun-spots.

Extra Observations.—The Solar-radiation Thermometers are still observed daily, and a new form of the instrument designed by Professor G. C. Foster, is at present undergoing trial.

The question of observing Solar Radiation having been referred by the Meteorological Council to the Kew Committee, a sub-committee has been appointed to take the whole subject into consideration.

The Campbell Sundial described in the 1875 Report, continues in action, and the improved form of the instrument, giving a separate record for every day, of the duration of sunshine, has been regularly worked throughout the year and its curves tabulated.

A paper comparing the relative amount of sunshine recorded by this instrument during the year 1877, with the amount registered at the Royal Observatory, Greenwich, by a similar apparatus, has been read by the Superintendent before the Meteorological Society, and published in their Quarterly Report, vol. iv, No. 28. It shows that the difference in the total duration of sunshine observed at the two stations, which amounted to 171 hours in the year, was in great measure due to the preponderance of westerly winds, which carry the smoke of the metropolis over the Royal Observatory.

A copy of the Kew instrument, constructed by Mr. Browning for the Brussels Observatory, has been compared at the Observatory; and another instrument, with a new form of mounting, designed by Mr. R. J. Lecky, F.R.A.S., is at present being tried.

Wind Component Integrator.—This instrument, at the time of the last Report, was working temporarily, attached to the Kew Anemograph. This arrangement was found, however, to interfere with the regular action of the latter instrument, and accordingly its own cups and vane, sent over by Professor von Oettingen, have been fitted to it by Mr. R. W. Munro; and with the exception of a small period, during which it was under repair (one of the cups having been carried away by a high wind), it has been in good action. A comparison of its indications with those of the ordinary instrument will shortly be made.

Photo-nephoscope.—This instrument, designed by Professor Stokes and Mr. F. Galton for the purpose of photographing clouds at the time of their passage across the zenith, has been the subject of experiment for some time, with a view of its adoption as a means of trigo-

nometrically determining the height of clouds. The experiments are still in progress.

Verifications.—The Committee have to report that the work of this department of the Observatory is still increasing, and its field extending, the makers who send instruments for examination, from places both at home and abroad, is continually becoming more numerous.

The following magnetic instruments have been verified, and had their constants determined:—

A set of Magnetographs for the Brussels Observatory.
A Unifilar for Messrs. Negretti and Zambra.
A Unifilar for the Dutch Arctic Expedition.
A Dip-circle „ „
A Fox Circle „ „
An Azimuth Compass „ „
Two Azimuth Compasses for Mr. H. M. Stanley.
A Dip-circle for the Austro-Hungarian Government.

There have also been purchased on commission and verified:—a Unifilar and Dip-circle for the Marine Observatory, San Fernando, Spain; a pair of Dipping-needles for Professor Wild, St. Petersburg; a Unifilar, Dip-circle, and Fox Circle for Captain Carl Wille, Horten, Norway; a Dip-circle for Senhor Capello, Lisbon Observatory. There are also now undergoing verification a Dip-circle for Lieutenant van Hasselt; a Dip-circle for the Austro-Hungarian Government; a Unifilar, Dip-circle, and Fox Circle for Professor Greene.

Two Sextants have been verified.

The following meteorological instruments have been verified, this portion of the work being entrusted to Mr. T. W. Baker, assisted by Messrs. Foster, Constable, and Gunter:—

Barometers,	Standard	56
„	Marine and Station	137
	Total	193
Aneroids		29
Thermometers,	ordinary Meteorological	1435
„	Boiling-point Standards	47
„	Mountain	16
„	Clinical	2032
„	Solar radiation	65
	Total	3595

In addition, 134 Thermometers have been tested at the melting-point of mercury.

14 Standard Thermometers have been calibrated and divided.

The following miscellaneous instruments have also been verified :—

Hydrometers............................	356
Anemometer............................	1

A Barograph and Thermograph have been examined, and their scale values determined, for the Brussels Observatory; also a similar pair of instruments for the Zi-ka-Wei Observatory, and a Thermograph for the Japanese Government.

There are at present in the Observatory undergoing verification, 19 Barometers, 182 Thermometers, 4 Anemometers, and 1 Rain-gauge.

A number of Aneroid Barometers, of a new pattern, have been received from MM. Hottinger and Co., of Zürich, for comparison.

The "Hall Mark," figured in last report, has been etched, at the desire of the makers, upon a number of the Thermometers compared at the Observatory.

A Hydraulic Press especially constructed for the purpose of subjecting Deep sea Thermometers to pressures similar to those they experience when sunk to great depths, has been erected in the workshop, by Messrs. Hopkinson and Cope. It is capable of exerting a strain equal to 4 tons on the square inch. Several protected Thermometers have been found to stand this test successfully.

Air Thermometer.—The Committee are taking steps to obtain a standard air thermometer.

The old "Royal Society" Standard Barometer, with the flint and crown-glass tubes refilled by Negretti and Zambra, has been compared with the Kew standard daily for several months. Its scale has also been measured and its error determined.

Comparison of Standard Barometers.—The account of the comparison of the Standard Barometers at Greenwich and Kew, which resulted in proving a close agreement between the standards of the two Observatories, was published in the "Royal Society Proceedings," vol. xxvii, p. 76.

With a view to determine the source of small variations in the correction to the working standard of the Observatory (Newman 34) and the large Welsh's standards, numerous comparisons have been made between the instruments, from time to time, but as yet without success.

Professor B. Stewart has had similar series of readings made between the Owens College ordinary Standard Barometer and one after the Kew model, also filled by Welsh's system. The results tend to show a most close agreement between the two forms of instru-

ment. For a complete account of these experiments, see "Manchester Philosophical Society's Proceedings," vol. xvii, No. 10.

Freezing Point of Water.—In consequence of a communication from Dr. Guthrie as to the presence of cryohydrates in water lowering its freezing point, a series of experiments was made for determining the melting point of distilled-water ice, rainwater ice, clean pond ice, and the commercial ice used at the Observatory. It was found to be practically identical in all the specimens examined, the differences observed only amounting to a few hundredths of a degree Fahrenheit.

Waxed Paper, &c., supplied.—Waxed paper has been supplied to the following Observatories:—

Bombay.	Montsouris.
Brussels.	Radcliffe.
Coimbra.	Zi-Ka-Wei.

A supply of chemical and photographic material has also been procured for the Coimbra Observatory.

A set of lamps, for use with Magnetographs, has been supplied to the Mauritius Observatory.

Loan Exhibition.—The old instruments (with the exception of a Magnet, the property of the Royal Observatory, Greenwich, and a Unifilar Magnetometer) lent to the Science and Art Department, enumerated in the Report for 1876, remain for the present deposited in the galleries at South Kensington.

A Dip-circle, the property of Mr. Dover, has been withdrawn from the collection.

Workshop.—The several pieces of Mechanical Apparatus, such as the Whitworth Lathe and the Planing Machine, procured by Grants from either the Government Grant Fund or the Donation Fund, for the use of the Kew Observatory, have been kept in thorough order, and many of them are in constant, and others in occasional use at the Observatory, but the funds of the Committee do not at present allow of the employment of a mechanical assistant, although one is much needed.

Library.—During the year the Library has received, as presents, the publications of

11 English Scientific Societies and Institutions,
43 Foreign and Colonial Scientific Societies and Institutions.

Ventilation Experiments.—The Sanitary Institute of Great Britain having applied to the Committee for permission to use the experimental house (which was unoccupied at the time) for a series of experiments on the ventilating powers of cowls of different form, the Committee granted it, and a large number of observations were made by them, extending over several weeks. A second set, with other appliances, is now about to be instituted.

Observatory and Grounds.—The buildings and grounds have been kept in repair, and application has been made to Her Majesty's Commissioners of Woods and Forests for a repainting of the interior, six years having elapsed since it was last done.

The Committee have received, as a donation, from Commander Sebastian Gassiot, R.N., busts of his father, the late J. P. Gassiot, Esq., and General Sir E. Sabine.

The Experimental House and Magnetic Observatory have been painted externally.

Staff.—The Staff employed at Kew is as follows:—Mr. G. M. Whipple, B.Sc., Superintendent; T. W. Baker, First Assistant; J. Foster, J. W. Hawkesworth, H. M'Laughlin, F. G. Figg, R. W. F. Harrison, E. G. Constable, T. Gunter, J. Hillier, and J. Dawson.

Mr. C. Robinson resigned his appointment in March, and was succeeded by Mr. Hillier.

Visitors.—The Observatory has been honoured by the presence, amongst others, of:—

Mr. Blanford.
Mr. Chambers.
The Chinese Educational Mission.
Captain De La Haye.
Professor Everett.
Mons. Houzeau.
The Hydrographer of the Japanese Navy.
Lieutenant-General Sir J. H. Lefroy.
Professor E. Mascart, Directeur du Bureau Central Météorologique, Paris.
Captain von Obermayer.
Dr. Wijkander.

(Signed) WARREN DE LA RUE,
Vice-Chairman.

Abstract. *Kew Observatory Receipts and Payments Account from November* 1, 1877, *to October* 31, 1878.

Dr. RECEIPTS.

		£ s. d.
To Balance from 1876–77		501 6 5
Royal Society (Gassiot Trust)	£248 18 8	
" "	246 16 7	
		495 15 3
Meteorological Office	100 0 0	
" "	100 0 0	
" "	100 0 0	
" "	100 0 0	
		400 0 0
Meteorological Office, for Postages, &c.		15 5 8
Payment for Instruments on Commission		794 18 0
Sale of Waxed Paper		77 3 4
Verification Fees, Magnetic Instruments	93 19 0	
" " Electrical "	4 2 0	
" " Met. Office, B. Trade	55 11 0	
" " " Admiralty	40 2 0	
" " " Commissions	69 0 6	
" " Opticians, &c.	223 8 5	
Sale of Standard Thermometers	37 18 0	
Payments for Copying Registers	61 0 0	
		585 0 11
Mr. De la Rue for Sun-work		125 7 10
Sale of Photographic Residues		5 0 8
		£2999 18 1

PAYMENTS. *Cr.*

		£ s. d.
By Salaries and extra work		958 16 0
Rent of Land	£11 0 0	
Fuel and Gas	65 6 5	
Furniture and Fittings	18 4 10	
Chandlery, &c.	12 14 6	
Painting and Repairs	32 18 5	
		140 4 2
Printing and Stationery	47 0 9	
Postages	11 6 6	
Library	13 3 9	
Messenger and Housekeeper	57 4 0	
Porterage and Contingencies	30 17 3	
		159 12 3
Instruments purchased on Commission		786 13 7
Postages and Payments on behalf of Meteorological Office		10 6 4
Purchase of Waxed Paper, Packing ditto, &c.		63 12 0
" " Chemicals	19 3 0	
" " Thermometer-tubes	7 6 6	
" " Ice and Carbonic-Acid Gas	16 7 2	
" " Anemograph Sheets	2 2 6	
" " Instruments	58 9 0	
Repair of ditto	33 8 2	
Carpenter's Work and Sundries	6 16 2	
		143 12 6
Sun-work expenses		104 1 3
Expenses on behalf of Sanitary Institute of Great Britain	9 15 9	
" " Instruments for Experiments	2 11 8	
		12 7 5
Balance—London and Westminster Bank	525 15 5	
London and County Bank	80 0 0	
Cash in hand	14 17 2	
		620 12 7
		£2999 18 1

November 13, 1878. Examined and compared with the Vouchers, and found correct. (Signed) ROBERT H. SCOTT, *Auditor.*

ASSETS.

	£ s. d.
By Balance as per Statement	620 12 7
Meteorological Office, Allowances and Sundries	46 18 4
Commissions	5 13 0
Waxed Paper sold	9 10 0
" " in stock	26 5 0
Verification Fees due	57 19 2
Standard Thermometers sold	2 2 0
" " in stock	90 8 0
Sun-work	4 1 3
Sanitary Institute of Great Britain	10 4 1

LIABILITIES.

	£ s. d.
To Gas, Fuel, and House Account	22 8 8
Apparatus, Chemicals, and Waxed Paper	31 3 3
Balance	820 1 6

APPENDIX I.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N., Long.* $0^h\ 1^m\ 15^s{\cdot}1$ *W., for the year October* 1877 *to September* 1878.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones.

The Declination observations have also been made with the same Magnetometer, Collimator Magnet N E being employed for the purpose.

The Dip observations were made with Dip-circle No. 33, the needles 1 and 2 only being used; these are $3\frac{1}{2}$ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the unit in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the factor for reducing the English to metric values being 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log π^2K employed in the reduction is 1·64365 at temperature 60° F.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° F. is

$$0{\cdot}0001194(t_0-35)+0{\cdot}000{,}000{,}213(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 foot and 1·3 feet, are 1·000075 feet and 1·300097 feet respectively.

The times of vibration given in the Table are each derived from the mean of 12 or 14 observations of the time occupied by the magnet in making 100 vibrations, corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^2}\right)$, is $-0{\cdot}00179$.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected about a quarter of a mile north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, was determined by the late Mr. Welsh, and has since been carefully verified.

The observations have all been made and reduced by Mr. F. G. Figg.

Observations of Deflection for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Distances of Centres of Magnets.	Temperature.	Observed Deflection.	Log $\frac{m}{X}$. Mean.	Observer.
1877.	d. h. m.	foot.		° ′ ″		
October........	26 12 34 P.M.	1·0	55·5	15 38 6		F.
		1·3	....	7 3 8	9·13146	"
	2 38 "	1·0	58·8	15 37 3		"
		1·3	....	7 2 42		"
November......	27 12 31 P.M.	1·0	54·6	15 37 46		F.
		1·3	....	7 3 5	9·13131	"
	2 18 "	1·0	55·3	15 37 26		"
		1·3	....	7 2 45		"
December......	21 12 35 P.M.	1·0	43·6	15 38 37		F.
		1·3	....	7 3 18	9·13093	"
	2 30 "	1·0	45·4	15 38 4		"
		1·3	....	7 3 8		"
1878.						
January........	28 12 41 P.M.	1·0	45·3	15 39 26		F.
		1·3	....	7 3 41	9·13139	"
	2 34 "	1·0	43·6	15 39 23		"
		1·3	....	7 3 37		"
February	26 12 16 P.M.	1·0	50·9	15 37 26		F.
		1·3	....	7 2 47	9·13092	"
	2 13 "	1·0	52·7	15 37 14		"
		1·3	....	7 2 37		"
March	27 12 18 P.M.	1 0	46·6	15 39 24		F.
		1·3	....	7 3 45	9·13139	"
	2 26 "	1·0	52·2	15 37 55		"
		1·3	....	7 2 54		"
April..........	27 12 39 P.M.	1·0	63·1	15 33 32		F.
		1·3	....	7 2 12	9·13080	"
	2 25 "	1·0	62·7	15 35 42		"
		1·3	....	7 2 6		"
May	27 12 40 P.M.	1·0	66·8	15 34 23		F.
		1·3	....	7 1 32	9·13068	"
	2 35 "	1·0	68·2	15 34 12		"
		1·3	....	7 1 21		"
June	26 12 42 P.M.	1·0	87·4	15 31 55		F.
		1·3	....	7 0 16	9·13086	"
	2 39 "	1·0	89·7	15 31 2		"
		1·3	....	6 59 52		"
July	29 12 42 P.M.	1·0	72·8	15 34 7		F.
		1·3	....	7 1 22	9·13083	"
	2 37 "	1·0	72·5	15 33 26		"
		1·3	....	7 1 8		"
August	28 12 27 P.M.	1·0	71·3	15 33 52		F.
		1·3	....	7 1 10	9·13058	"
	2 39 "	1·0	70·6	15 33 16		"
		1·3	....	7 1 2		"
September......	24 12 33 P.M	1·0	57·5	15 36 1		F.
		1·3	....	7 2 16	9·13060	"
	2 37 "	1·0	60·1	15 34 47		"
		1·3	....	7 1 42		"

Vibration Observations for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Temperature.	Time of one Vibration.	Log mX. Mean.	Value of m.	Observer.
1877.	d. h. m.	°	secs.			
October	26 11 52 A.M.	54·2	4·6353			F.
	3 14 P.M.	61·1	4·6356	0·31152	0·52661	„
November	27 11 45 A.M.	53·5	4·6328			F.
	2 58 P.M.	54·9	4·6331	0·31187	0·52673	„
December	21 11 54 A.M.	42·0	4·6266			F.
	3 5 P.M.	46·0	4·6286	0·31224	0·52672	„
1878.						
January	28 12 0 noon.	44·5	4·6290			F.
	3 9 P.M.	44·0	4·6308	0·31182	0·52674	„
February	26 11 37 A.M.	49·7	4·6305			F.
	2 49 P.M.	52·4	4·6316	0·31200	0·52657	„
March	27 11 31 A.M.	44·3	4·6331			F.
	3 4 P.M.	54·7	4·6338	0·31133	0·52645	„
April	27 11 21 A.M.	60·7	4·6365			F.
	3 2 P.M.	65·4	4·6366	0·31166	0·52630	„
May	27 11 53 A.M.	65·4	4·6383			F.
	3 13 P.M.	70·1	4·6381	0·31188	0·52636	„
June	26 11 56 A.M.	88·1	4·6454			F.
	3 15 P.M.	90·2	4·6454	0·31195	0·52650	„
July	29 11 57 A.M.	73·6	4·6418			F.
	3 17 P.M.	73·0	4·6398	0·31175	0·52637	„
August	28 11 45 A.M.	71·3	4·6407			F.
	3 15 P.M.	71·0	4·6385	0·31184	0·52627	„
September	24 11 52 A.M.	56·2	4·6361			F.
	3 11 P.M.	61·4	4·6340	0·31188	0·52631	„

Dip Observations.

Month.	G. M. T.	Needle.	Dip.	Observer.	Month.	G. M. T.	Needle.	Dip.	Observer.
			North.					North.	
1877.	d. h. m.	No.	° ′		1878.	d. h. m.	No.	° ′	
Oct.	29 3 8 P.M.	1	67 45·97	F.	Apl.	29 3 4 P.M.	1	67 44·68	F.
	3 10 „	2	45·19	„		3 1 „	2	44·12	„
						30 3 29 „	1	44·62	„
						3 27 „	2	43·00	„
	Mean..	....	67 45·58			Mean..	....	67 44·10	
Nov.	28 2 56 P.M.	1	67 44·31	F.	May	29 3 7 P.M.	1	67 43·81	F.
	2 57 „	2	44·16	„		3 7 „	2	44·06	„
	30 2 49 „	1	44·75	„		30 3 9 „	1	44·68	„
	2 48 „	2	44·09	„		3 12 „	2	44·12	„
	Mean..	....	67 44·33			Mean..	....	67 44·17	
Dec.	20 2 55 P.M.	1	67 44·87	F.	June	27 3 19 P.M.	1	67 42·12	F.
	2 54 „	2	44·06	„		3 17 „	2	41·37	„
	24 2 50 „	1	44·87	„		28 3 7 „	1	43·81	„
	2 51 „	2	44·06	„		3 8 „	2	41·81	„
	Mean..	....	67 44·46			Mean..	....	67 42·28	
1878.									
Jan.	29 2 55 P.M.	1	67 45·12	F.	July	30 3 6 P.M.	1	67 43·06	F.
	2 55 „	2	44·19	„		3 5 „	2	41·32	„
	30 2 53 „	1	44·81	„		31 3 4 „	1	44·00	„
	2 54 „	2	44·50	„		3 5 „	2	43·00	„
	Mean..	....	67 44·65			Mean..	....	67 42·84	
Feb.	27 2 50 P.M.	1	67 44·75	F.	Aug.	29 3 15 P.M.	1	67 43·68	F.
	2 48 „	2	43·50	„		3 15 „	2	43·00	„
	28 3 0 „	1	45·12	„		30 3 15 „	1	43·31	„
	3 0 „	2	44·31	„		3 15 „	2	43·18	„
	Mean..	...	67 44·42			Mean..	....	67 43·29	
Mar.	28 2 54 P.M.	1	67 44·69	F.	Sept.	25 3 2 P.M.	1	67 43·58	F.
	2 53 „	2	43·94	„		3 1 „	2	43·43	„
	29 2 57 „	1	44·68	„		26 3 0 „	1	43·75	„
	2 57 „	2	43·81	„		3 1 „	2	43·81	„
	Mean..	....	67 44·28			Mean..	....	67 43·63	

Declination Observations.

Month.	G. M. T.	Uncorrected.		Corrected for Torsion.		Observer.
		Observation.	Monthly Mean.	Observation.	Monthly Mean.	
		West.	West.	West.	West.	
1877.	d. h. m.	° ′ ″		° ′ ″		
ober	29 12 34 P.M.	19 18 56		19 18 56		F.
	30 12 33 „	19 20 28	° ′ ″ 19 19 42	19 20 28	° ′ ″ 19 19 42	„
ember ..	28 12 27 „	19 15 52		19 17 53		F.
	29 12 34 „	19 16 48	19 16 20	19 16 48	19 17 20	„
ember ..	22 12 35 „	19 15 21		19 16 32		F.
	24 12 29 „	19 16 50	19 16 5	19 16 50	19 16 41	„
1878. uary....	29 12 22 „	19 16 40		19 16 40		F.
	30 12 31 „	19 13 25	19 15 2	19 14 45	19 15 42	„
ruary. ..	27 12 28 „	19 21 20		19 18 50		F.
	28 12 40 „	19 17 23	19 19 21	19 16 33	19 17 41	„
rch	28 12 30 „	19 11 33		19 13 29		F.
	29 12 29 „	19 17 36	19 14 34	19 15 39	19 14 34	„
ril	29 12 29 „	19 13 47		19 10 59		F.
	30 12 36 „	19 14 25	19 14 6	19 17 12	19 14 5	„
y	29 12 30 „	19 12 9		19 13 36		F.
	30 12 42 „	19 17 46	19 14 57	19 17 46	19 15 41	„
ıe.......	27 12 30 „	19 15 10		19 17 51		F.
	28 12 35 „	19 19 6	19 17 8	19 20 53	19 19 22	„
y	30 12 19 „	19 12 52		19 12 18		F.
	31 12 30 „	19 13 32	19 13 12	19 12 35	19 12 27	„
gust.....	29 12 33 „	19 17 44		19 15 59		F.
	31 12 40 „	19 12 9	19 14 56	19 13 53	19 14 56	„
tember..	25 12 27 „	19 9 50		19 12 10		F.
	26 12 23 „	19 13 33	19 11 41	19 13 33	19 12 51	„

Magnetic Intensity.

Month.	English Units.			Metric Units.			C. G. S. Measure.		
	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.
1877. October..	3·8907	9·5148	10·2795	1·7939	4·3871	4·7397	0·1794	0·4387	0·4740
November	3·8930	9·5104	10·2764	1·7950	4·3851	4·7383	0·1795	0·4385	0·4738
December	3·8963	9·5196	10·2861	1·7965	4·3893	4·7427	0·1796	0·4389	0·4743
1878. January..	3·8923	9·5113	10·2771	1·7947	4·3855	4·7386	0·1795	0·4385	0·4739
February.	3·8953	9·5168	10·2830	1·7961	4·3880	4·7413	0·1796	0·4388	0·4741
March ...	3·8902	9·5032	10·2686	1·7937	4·3818	4·7347	0·1794	0·4382	0·4735
April	3·8943	9·5117	10·2783	1·7956	4·3857	4·7391	0·1796	0·4386	0·4739
May	3·8958	9·5161	10·2828	1·7963	4·3877	4·7412	0·1796	0·4388	0·4741
June.....	3·8953	9·4999	10·2676	1·7961	4·3803	4·7342	0·1796	0·4380	0·4734
July	3·8946	9·5025	10·2698	1·7957	4·3815	4·7352	0·1796	0·4381	0·4735
August...	3·8961	9·5098	10·2768	1·7964	4·3848	4·7385	0·1796	0·4385	0·4738
September	3·8962	9·5128	10·2797	1·7965	4·3862	4·7398	0·1796	0·4386	0·4740

APPENDIX II.

Meteorological Observations.—Table I.

Kew Observatory.

Longitude 0h 1m 15s·1 W. Latitude 51° 28′ 6″ N. Height above sea-level =34 feet.

Mean Monthly results from the continuous Records for the Twelve Months ending September 30th, 1878.

Months.	Thermometer.* Means.	Thermometer.* Extreme maximum. Date.	Thermometer.* Extreme maximum. Ther.	Thermometer.* Extreme minimum. Date.	Thermometer.* Extreme minimum. Ther.	Barometer.† Means.	Barometer.† Extreme maximum. Date.	Barometer.† Extreme maximum. Bar.	Barometer.† Extreme minimum. Date.	Barometer.† Extreme minimum. Bar.	Pressure. Means. Vapour-tension.	Pressure. Means. Dry air.
	°	d. h.	°	d. h.	°	inches.	d. h.	inches.	d. h.	inches.	inch.	inches.
1877. October....	48·5	14 1 P.M.	65·0	18 8 A.M.	29·0	30·027	6 8 A.M.	30·680	25 2 P.M.	29·212	·279	29·748
November..	45·5	16 11 A.M.	58·0	4 4 ,,	29·0	29·689	17 1 ,,	30·474	11 11 ,,	28·718	·262	29·427
December..	41·0	6 2 P.M.	53·3	28 7 ,,	27·9	30·044	20 9 ,,	30·691	1 2 A.M.	29·138	·223	29·821
1878. January....	40·8	21 3 ,, 22 2 ,,	54·7	11 10 P.M.	26·4	30·167	12 10 ,,	30·703	25 9 ,,	29·376	·217	29·950
February...	42·5	17 2 ,,	57·5	8 9 A.M.	25·9	30·285	22 1 ,,	30·653	27 8 P.M.	29·659	·238	30·047
March.....	42·8	7 4 ,,	57·1	26 7 ,,	27·5	30·075	16 11 ,, 16 9 P.M.	30·666	29 5 A.M.	29·178	·204	29·871
April......	48·4	30 1 ,,	64·6	1 6 ,,	29·0	29·838	27 9 ,,	30·234	1 5 ,,	29·139	·266	29·572
May	55·0	10 3 ,,	71·2	21 4 ,,	39·1	29·794	30 10 A.M.	30·166	24 2 P.M. 24 4 ,,	29·292	·347	29·447
June......	60·3	26 2 ,,	85·6	2 3 ,,	44·0	29·943	6 11 ,,	30·247	11 4 ,,	29·485	·394	29·549
July.......	63·1	19 2 ,,	81·8	4 4 ,,	47·1	30·036	31 0 midt.	30·373	24 4 ,,	29·654	·428	29·608
August....	62·2	5 3 ,,	77·1	26 4 ,,	51·9	29·759	1 1 A.M.	30·373	30 6 A.M.	29·154	·447	29·312
September..	56·3	7 4 ,,	71·9	24 6 ,,	37·0	29·995	2 0 noon	30·309	23 3 P.M.	29·463	·374	29·621
	50·5	....	..	....	..	29·971	....	..	....	..	·307	29·664

The above Table is extracted from the Quarterly Weather Report of the Meteorological Office, by permission of the Meteorological Council.

* The thermometer-bulbs are 10 feet above the ground. † Readings reduced to sea-level.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall *.			Weather †. Number of days of						Wind ‡. Number of days on which it blew							
		Total.	Maximum.	Date.	Rain.	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1877.		in.	in.															
October ..	6·1	1·985	0·310	24	15	..	..	..	3	10	4	3	2	..	4	12	4	2
November	6·3	3·490	1·020	11	21	..	1	..	..	9	..	1	..	1	11	11	2	4
December	7·0	1·300	0·468	28	13	1	..	..	3	14	3	..	1	1	4	8	7	7
1878.																		
January ..	7·6	1·185	0·230	3 & 27	13	1	..	..	2	16	4	3	2	1	3	7	6	5
February .	8·3	1·235	0·370	13	10	..	..	..	..	17	2	1	3	2	5	8	5	2
March ...	7·7	1·030	0·580	28	5	3	..	..	..	15	6	3	2	..	1	4	7	8
April	6·7	3·990	1·190	10	17	..	2	2	1	12	3	3	9	1	5	6	2	1
May.....	7·8	4·005	1·100	28	20	..	1	4	..	15	2	2	4	1	4	12	5	1
June	6·6	2·745	0·780	18	15	..	..	6	2	10	2	4	4	3	2	12	2	1
July.....	7·0	2·345	1·165	24	9	..	..	3	2	13	5	6	5	..	1	3	6	5
August ..	7·4	6·500	1·400	3	22	..	..	6	1	16	..	3	6	1	5	11	4	1
September	6·3	0·970	0·250	17	14	..	..	..	2	6	2	1	1	..	1	10	9	6
Total..		30·780			174	5	4	21	16	153	33	30	39	11	46	104	59	43

* Measured daily at 10 A.M. by gauge 1·75 feet above surface of ground. † Derived from observations made at 10 A.M., noon, 2, 4, and 10 P.M.
‡ As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Sunshine.*		Maximum temperature in sun's rays.†			Horizontal movement of the Air.‡		
	Total number of hours.	Number of hours sun was above the horizon.	Mean.	Highest.	Date.	Average daily Velocity.	Greatest Movement in a day.	Date.
1877.	h. m.	h. m.				miles.	miles.	
October	120 12	328 26	100°·2	110°·0	1	242	579	15
November	81 18	264 8	88·9	104·8	5	284	686	11
December	54 30	242 55	68·7	88·0	9	217	436	24
1878.								
January	51 12	259 7	70·7	91·2	16	245	631	21
February	44 42	277 46	75·5	103·9	18	180	399	27
March	107 24	366 49	98·6	108·2	15	284	695	29
April	155 24	414 33	112·4	128·0	29	284	694	8
May	130 36	426 4	120·7	133·1	9	275	544	19
June	149 6	494 22	128·1	146·8	26	193	469	11
July	185 24	497 17	130·0	140·5	14	169	339	20 and 21
August	178 30	449 31	127·0	142·3	18	238	436	2
September	149 36	377 40	115·2	136·4	4	165	463	15

* Registered by Campbell's sun-dial.

† Derived from the means of the indications of four black-bulb thermometers in vacuo, read daily at 10 A.M.

‡ As indicated by Robinson's anemograph, 70 feet above the general surface of ground.

Erratum in Report for 1877, p. 16, note, for "unreduced" read "reduced."

Presents, November 21, 1878.

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December 5, 1878.

W. SPOTTISWOODE, M.A., D.C.L., President, in the Chair.

The President announced that he had appointed as Vice-Presidents:—

The Treasurer.
Mr. Justice Grove.
Sir Joseph Hooker.
Lord Lindsay.
Sir John Lubbock.

The Hon. Sir James Cockle (elected in 1865) and Lord Lindsay were admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Illumination of Lines of Molecular Pressure, and the Trajectory of Molecules." By WILLIAM CROOKES, F.R.S., V.P.C.S. Received November 30, 1878.

(Abstract.)

Induction Spark through Rarefied Gases. Dark Space round the Negative Pole.

The author has examined the dark space which appears round the negative pole of an ordinary vacuum tube when the spark from an induction coil is passed through it. He describes many experiments with different kinds of poles, a varying intensity of spark, and different gases, and arrives at the following propositions.

Illumination of Lines of Molecular Pressure.

a. Setting up an intense molecular vibration in a disk of metal by electrical means excites a molecular disturbance which affects the surface of the disk and the surrounding gas. With a dense gas the disturbance extends a short distance only from the metal; but as rarefaction continues the layer of molecular disturbance increases in thickness. In air at a pressure of ·078 millim. this molecular disturbance extends for at least 8 millims. from the surface of the disk, forming an oblate spheroid around it.

b. The diameter of this dark space varies with the exhaustion; with the kind of gas in which it is produced; with the temperature of the negative pole; and, in a slight degree, with the intensity of the spark. For equal degrees of exhaustion it is greatest in hydrogen and least in carbonic acid, as compared with air.

c. The shape and size of this dark space do not vary with the distance separating the poles; nor, only very slightly, with alteration of battery power, or with intensity of spark. When the power is great the brilliancy of the unoccupied parts of the tube overpowers the dark space, rendering it difficult of observation; but, on careful scrutiny, it may still be seen unchanged in size, nor does it alter even when, with a very faint spark, it is scarcely visible. On still further reduction of the power it fades entirely away, but without change of form.

The author describes numerous experiments, devised to ascertain if this visible layer of molecular disturbance is identical with the invisible layer of molecular pressure or stress, the investigation of which has occupied him for some years.

The Electrical Radiometer.

One of these experiments is as follows:—An ordinary radiometer is made, with aluminium disks for vanes, each disk coated with a

film of mica. The fly is supported by a hard steel cup instead of a glass cup, and the needle point on which it works is connected by means of a wire with a platinum terminal sealed into the glass. At the top of the radiometer bulb a second terminal is sealed in. The radiometer can therefore be connected with an induction coil, the movable fly being made the negative pole.

Passing over the phenomena observed at low exhaustions, the author finds that, when connected with the coil, a halo of a velvety violet light forms on the metallic side of the vanes, the mica side remaining dark throughout these experiments. As the pressure diminishes, a dark space is seen to separate the violet halo from the metal. At a pressure of half a millim. this dark space extends to the glass, and positive rotation commences.

On continuing the exhaustion, the dark space further widens out and appears to flatten itself against the glass, and the rotation becomes very rapid.

When aluminium cups are used for the vanes instead of disks backed with mica, similar appearances are seen. The velvety violet halo forms over each side of the cup. On increasing the exhaustion the dark space widens out, retaining almost exactly the shape of the cup. The bright margin of the dark space becomes concentrated at the concave side of the cup to a luminous focus, and widens out at the convex side. On further exhaustion, the dark space on the convex side touches the glass, when positive rotation commences, becoming very rapid as the dark space further increases in size, and ultimately flattening against the glass.

Convergence of Molecular Rays to a Focus.

The subject next investigated is the convergence of the lines of force to a focus, as observed with the aluminium cup. As this could not be accomplished during rapid rotation, an instrument was made, having the cup-shaped negative pole fixed, instead of movable. On exhaustion, the convergence of the lines of force to a focus at the concave side was well observed. When the dark space is very much larger than the cup, it forms an irregular ellipsoid drawn in towards the focal point. Inside the luminous boundary a focus of dark violet light can be seen converging, and, as the rays diverge on the other side of the focus, spreading beyond the margin of the dark space; the whole appearance being strikingly similar to the rays of the sun reflected from a concave mirror through a foggy atmosphere.

Green Phosphorescent Light of Molecular Impact.

At very high exhaustions the dark space becomes so large that it fills the tube. Careful scrutiny still shows the presence of the dark violet focus; and the part of the glass on which fall the rays diverging

from this focus shows a sharply-defined spot of greenish-yellow light. On still further exhaustion, and especially if the cup is made positive, the bulb becomes beautifully illuminated with greenish-yellow phosphorescent light.

This greenish-yellow phosphorescence, characteristic of high exhaustions, is frequently spoken of in the paper. It must be remembered, however, that the particular colour is due to the special kind of soft German glass used. Other kinds of glass phosphoresce in a different colour. The phosphorescence takes place only under the influence of the rays from the negative pole. At an exhaustion of 4 M*, no light other than this is seen in the apparatus. At ·9 M the phosphorescence is about at its maximum. When the exhaustion reaches ·15 M the spark has a difficulty in passing, and the green light appears occasionally in flashes only. At ·06 M the vacuum is almost non-conductive, and a spark can be forced through only by increasing the intensity of the coil, and well insulating the tube and wires leading to it. Beyond that exhaustion nothing has been observed.

Focus of Molecular Energy.

In an apparatus specially constructed for observing the position of the focus, the author found that the focal point of the green phosphorescent light was at the centre of curvature, showing that the molecules by which it is produced are projected in a direction normal to the surface of the pole. Before reaching the best exhaustion for the green light, another focus of blue-violet light is observed; this varies in position, getting further from the pole as the exhaustion increases. In the apparatus described, at an exhaustion of 19·3 M, these two foci are seen simultaneously, the green being at the centre of curvature, while the blue focus is at nearly twice the distance.

Nature of the Green Phosphorescent Light.

The author adduces the following characteristics of the green phosphorescent light, as distinguishing it from the ordinary light observed in vacuum-tubes at lower exhaustions:—

a. The green focus cannot be seen in the space of the tube, but where the projected beam strikes the glass only.

b. The position of the positive pole in the tube makes scarcely any difference to the direction and intensity of the lines of force which produce the green light. The positive pole may be placed in the tube either at the extremity opposite the negative pole, or below it, or by its side.

c. The spectrum of the green light is a continuous one, most of the red and the higher blue rays being absent; while the spectrum of the light observed in the tube at lower exhaustions is characteristic of the

* M signifies the millionth of an atmosphere.

residual gas. No difference can be detected by spectrum examination in the green light, whether the residual gas be nitrogen, hydrogen, or carbonic acid.

d. The green phosphorescence commences at a different exhaustion in different gases.

e. The viscosity of a gas is almost as persistent a characteristic of its individuality as its spectrum. The author refers to a preliminary note and a diagram* of the variation of viscosity of air, hydrogen, and other gases at exhaustions between 240 **M** and ·1 **M**. From these and other unpublished results, the author finds that the viscosity of a gas undergoes very little diminution between atmospheric pressure and an exhaustion at which the green phosphorescence can be detected. When, however, the spectral and other characteristics of the gas begin to disappear, the viscosity also commences to decline, and at an exhaustion at which the green phosphorescence is most brilliant, the viscosity has rapidly sunk to an insignificant amount.

f. The rays exciting green phosphorescence will not turn a corner in the slightest degree, but radiate from the negative pole in straight lines, casting strong and sharply defined shadows from objects which happen to be in their path. On the other hand, the ordinary luminescence of vacuum tubes will travel hither and thither along any number of curves and angles.

Projection of Molecular Shadows.

The author next examines the phenomena of shadows cast by the green light. The best and sharpest shadows are cast by flat disks and not by narrow pointed poles; no green light whatever is seen in the shadow itself, no matter how thin, or whatever may be the substance from which it is thrown.

From these and other experiments, fully described in the paper, he ventures to advance the theory that the induction spark actually illuminates the lines of molecular pressure caused by the electrical excitement of the negative pole. The thickness of the dark space is the measure of the length of the path between successive collisions of the molecules. The extra velocity with which the molecules rebound from the excited negative pole keep back the more slowly moving molecules which are advancing towards that pole. The conflict occurs at the boundary of the dark space, where the luminous margin bears witness to the energy of the discharge.

When the exhaustion is sufficiently high for the length of path between successive collisions to be greater than the distance between the fly and the glass, the swiftly moving rebounding molecules spend their energy, in part or in whole, on the sides of the vessel, and the

* "Proc. Poy. Soc.," Nov. 16, 1876, vol. xxv, p. 305.

production of light accompanies this sudden arrest of velocity. The light proceeds from the glass, and is apparently caused by fluorescence or phosphorescence on its surface. No light is produced by a mica or quartz screen, and the more fluorescent the material the better the luminosity. Here the consideration arises that the greenish-yellow light is an effect of the arrest of the negatively electrified molecules by the surface of the glass; but whether they actually strike the glass, or whether at the boundary surface separating solid from gaseous matter, there are intermediary layers of condensed gas which, taking up the blow, pass it on to the layer beneath, are problems the solution of which must be left to further research. The shadows are not optical, but are molecular shadows, revealed only by an ordinary illuminating effect; this is proved by the sharpness of the shadow when projected from a wide pole.

Phosphorescence of Thin Films.

An experiment is next described in which a film of uranium glass, sufficiently thin to show colours of thin plates, is placed in front of a thick plate of the same glass; the whole being enclosed in a tube with terminals, and exhausted to a few millionths of an atmosphere. Of this the following observations are recorded :—

a. The uranium film, being next to the negative pole, casts a strong shadow on the plate.

b. On making contact with the coil, the thin film flashes out suddenly all over its surface with a yellowish phosphorescence, which, however, instantly disappears. The uncovered part of the plate does not become phosphorescent quite suddenly, but the phosphorescence is permanent as long as the coil is kept at work.

c. With an exceedingly faint spark the film remains more luminous than the plate, but on intensifying the spark, the luminosity of the film sinks and that of the uncovered part of the plate increases.

d. If a single intense spark be suddenly sent through the tube, the film becomes very luminous, while the plate remains dark.

These experiments are conclusive against the phosphorescence being an effect of the radiation of phosphorogenic ultra-violet light from a thin layer of arrested molecules at the surface of the glass, for were this the case, the film could under no circumstances be superior to the plate.

The momentary phosphorescence and rapid fading of the film prove more than this. The molecular bombardment is too much for the thin film. It responds thereto at first, but immediately gets heated by the impacts, and then ceases to be luminous. The plate, however, being thick, bears the hammering without growing hot enough to lose its power of phosphorescing.

Mechanical Action of Projected Molecules.

When the coil was first turned on, the thin film was driven back at the moment of becoming phosphorescent, showing that an actual material blow had been given by the molecules. Experiments are next described in which this mechanical action is rendered more evident. A small rotating fly, capable cf being moved about in any part of an exhausted bulb, is used as an ndicator, and by appropriate means the molecular shadow of an aluminium plate is projected along the bulb. Whether entirely in, or entirely out of the shadow, the indicator searcely moves, but when immersed so that one-half is exposed to molecular impact the fly rotates with extreme velocity.

Magnetic Deflection of Lines of Molecular Force.

With this apparatus another phenomenon was investigated. It is found that the stream of molecules, whose impact on the glass is accompanied by evolution of light, is very sensitive to magnetic influence, and by bringing one pole of an electro-magnet—or even of a small permanent magnet—near, the shadow can be twisted to the right or to the left.

When the little indicator was placed entirely within the molecular shadow, no movement was produced. As soon, however, as an adjacent electro-magnet was excited, the shadow was deflected half off the indicator, which immediately rotated with great speed.

The Trajectory of Molecules.

The amount of deflection of the stream of molecules forming a shadow is in proportion to the magnetic power employed.

The trajectory of the molecules forming the shadow is curved when under magnetic influence; the action of the magnet is to twist the trajectory of the molecules round in a direction at an angle to their free path, and to a greater extent, as they are nearer the magnet: the direction of twist being that of the electric current passing round the electro-magnet.

Laws of Magnetic Deflection.

An apparatus was constructed so that the deflection of a spot of light was observed instead of that of a shadow; a horseshoe magnet being placed underneath the negative pole to deflect the trajectory. The action of the north pole being to give the ray of molecules a spiral twist one way, and that of the south pole being to twist it the other way, the two poles side by side compel the ray to move in a straight line up or down, along a plane at right angles to the plane of the magnet and a line joining its poles.

The ray of molecules does not appear to obey Ampère's law, as it would were it a perfectly flexible conductor, joining the negative and the posi-

tive pole. The molecules are projected from the negative, but the position of the positive pole—whether in front, at the side, or even behind the negative pole—has no influence on their subsequent behaviour, either in producing phosphorescence, or mechanical effects, or in their magnetic deflection. The magnet gives their line of path a spiral twist, greater or less according to its power, but diminishing as the molecules get further off.

Numerous experiments were tried in this apparatus with different gases, and with the magnet in and out of position.

Working with exhausted air it was found that the spot of green phosphorescence on the screen is visible at an exhaustion of 102·6 M, when the free path of the molecules, measured by the thickness of the dark space round the negative pole, is only 12 millims. Hence, it follows, that a number of molecules sufficient to excite green phosphorescence on the screen are projected the whole distance from the pole to the screen, or 102 millims., without being stopped by collisions.

Alteration of Molecular Velocity.

If we suppose the horseshoe magnet to be permanently in position, and thus to exert a uniform downward pressure on the molecules, we perceive that their trajectory is much curved at low exhaustions, and gets flatter as the exhaustion increases. A flatter trajectory corresponds to a higher velocity. This may arise from one of two conditions; either the initial impulse given by the negative pole is stronger, or the molecules experience less resistance. The latter is probably the true one. The molecules which produce the green phosphorescence must be looked upon as in a state differing from those arrested by frequent collisions. Any action which impedes the velocity of the free molecules allows longer time for the magnetism to act on them; and, although the deflecting force of magnetism might be expected to decrease with the velocity of the molecules, Professor Stokes has pointed out that it would have to decrease as the *square* of the velocity, in order that the deflection should be no greater at low than at high velocities.

Comparing the free molecules to cannon balls, the magnetic deflection to the earth's gravitation, and the electrical excitation of the negative pole to the explosion of the powder in the gun, the trajectory will be flat when no gravitation acts, and curved when under the influence of gravitation. It is also much curved when the ball passes through a dense resisting medium; it is less curved when the resisting medium gets rarer; and, as already shown, intensifying the induction spark, equivalent to increasing the charge of powder, gives greater initial velocity, and therefore flattens the trajectory. The parallelism is still closer if we compare the evolution of light seen when the shot

strikes the target, with the phosphorescence on the glass screen accompanying molecular impacts.

Focus of Heat of Molecular Impact.

The author finally describes an apparatus in which he shows that great heat is evolved when the concentrated focus of rays from a nearly hemispherical aluminium cup is deflected sideways by a magnet to the walls of the glass tube. By using a somewhat larger hemisphere and allowing the negative focus to fall on a strip of platinum foil, the heat rises to the melting point of platinum.

An Ultra-gaseous State of Matter.

The paper concludes with some theoretical speculations on the state in which the matter exists in these highly exhausted vessels. The modern idea of the gaseous state is based upon the supposition that a given space contains millions of millions of molecules in rapid movement in all directions, each having millions of encounters in a second. In such a case, the length of the mean free path of the molecules is exceedingly small as compared with the dimensions of the vessel, and the properties which constitute the ordinary gaseous state of matter, which depend upon constant collisions, are observed. But by great rarefaction the free path is made so long that the hits in a given time may be disregarded in comparison to the misses, in which case the average molecule is allowed to obey its own motions or laws without interference; and if the mean free path is comparable to the dimensions of the vessel, the properties which constitute gaseity are reduced to a minimum, and the matter becomes exalted to an ultragaseous state, in which the very decided but hitherto masked properties now under investigation come into play.

Rays of Molecular Light.

In speaking of a ray of molecular light, the author has been guided more by a desire for conciseness of expression than by a wish to advance a novel theory. But he believes that the comparison, under these special circumstances, is strictly correct, and that he is as well entitled to speak of a ray of molecular or emissive light when its presence is detected only by the light evolved when it falls on a suitable screen, as he is to speak of a sunbeam in a darkened room as a ray of vibratory or ordinary light when its presence is to be seen only by interposing an opaque body in its path. In each case the invisible line of force is spoken of as a ray of light, and if custom has sanctioned this as applied to the undulatory theory, it cannot be wrong to apply the expression to emissive light. The term emissive light must, however, be restricted to the rays between the negative pole and the luminous

screen: the light by which the eye then sees the screen is, of course, undulatory.

The phenomena in these exhausted tubes reveal to physical science a new world—a world where matter exists in a fourth state, where the corpuscular theory of light holds good, and where light does not always move in a straight line; but where we can never enter, and in which we must be content to observe and experiment from the outside.

II. "On a Machine for the Solution of Simultaneous Linear Equations." By Sir WILLIAM THOMSON, LL.D., F.R.S., President of the Royal Society, Edinburgh. Received August 30, 1878.

Let B_1, B_2, . . . B_n be n bodies each supported on a fixed axis (in practice each is to be supported on knife-edges like the beam of a balance).

Let P_{11}, P_{21}, P_{31}, . . . P_{n1}, n pulleys each pivoted on B_1;
P_{12}, P_{22}, P_{32}, . . . P_{n2} ,, ,, B_2;
P_{13}, P_{23}, P_{33}, . . . P_{n3} ,, ,, B_3;
. . . .
C_1, C_2, C_3, . . . C_n, n cords passing over the pulleys;
D_1, P_{11}, P_{12}, P_{13}, . . . P_{1n}, E_1, the course of C_1;
D_2, P_{21}, P_{22}, P_{23}, . . . P_{2n}, E_2, ,, C_2;
. . . .
D_1, E_1, D_2, E_2, . . . D_n, E_n, fixed points;

l_1, l_2, l_3, . . . l_n the lengths of the cords between D_1, E_1, and D_2, E_2, . . . and D_n, E_n, along the courses stated above, when B_1, B_2, . . . B_n, are in particular positions which will be called their zero positions;

l_1+e_1, l_2+e_2, . . . l_n+e_n, their lengths between the same fixed points, when B_1, B_2, . . . B_n are turned through angles x_1, x_2, . . . x_n from their zero positions;

$$\begin{array}{l} (11), (12), (13), \dots (1n), \\ (21), (22), (23), \dots (2n), \\ (31), (32), (33), \dots (3n), \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{array}$$

quantities such that

$$\left.\begin{array}{l} (11)x_1+(12)x_2+ \dots +(1n)x_n=e_1 \\ (21)x_1+(22)x_2+ \dots +(2n)x_n=e_2 \\ (31)x_1+(32)x_2+ \dots +(3n)x_n=e_3 \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ (n1)x_1+(n2)x_2+ \dots +(3n)x_n=e_n \end{array}\right\} \quad \dots \quad (\mathrm{I})$$

We shall suppose x_1, x_2, . . . x_n to be each so small that (11), (12),

. . . (21), &c., do not vary sensibly from the values which they have where $x_1, x_2, \ldots x_n$, are each infinitely small. In practice it will be convenient to so place the axes of $B_1, B_2, \ldots B_n$, and the mountings of the pulleys on $B_1, B_2, \ldots B_n$, and the fixed points D_1, E_1, D_2, &c., that when $x_1, x_2, \ldots x_n$ are infinitely small, the straight parts of each cord and the lines of infinitesimal motion of the centres of the pulleys round which it passes are all parallel. Then $\frac{1}{2}(11)$, $\frac{1}{2}(21)$, . . . $\frac{1}{2}(n)$ will be simply equal to the distances of the centres of the pulleys $P_{11}, P_{21}, \ldots P_n$, from the axis of B_1;

$\frac{1}{2}(12)$, $\frac{1}{2}(22)$ . . . $\frac{1}{2}(n2)$ the distances of $P_{12}, P_{22}, \ldots P_{n2}$ from the axis of B_2, and so on.

In practice the mounting of the pulleys are to be adjustable by proper geometrical slides, to allow any prescribed positive or negative value to be given to each of the quantities (11), (12), . . . (21), &c.

Suppose this to be done, and each of the bodies $B_1, B_2, \ldots B_n$ to be placed in its zero position and held there. Attach now the cords firmly to the fixed points $D_1, D_2, \ldots D_n$ respectively; and passing them round their proper pulleys, bring them to the other fixed points $E_1, E_2, \ldots E_n$, and pass them through infinitely small smooth rings fixed at these points. Now hold the bodies $B_1, B_2, \ldots$ each fixed, and (in practice by weights hung on their ends, outside $E_1, E_2, \ldots E_n$) pull the cords through $E_1, E_2, \ldots E_n$ with any given tensions* $T_1, T_2, \ldots T_n$. Let $G_1, G_2, \ldots G_n$ be moments round the fixed axes of $B_1, B_2, \ldots B_n$ of the forces required to hold the bodies fixed when acted on by the cords thus stretched. The principle of "virtual velocities," just as it came from Lagrange (or the principle of "work"), gives immediately, in virtue of (I),

$$\left.\begin{array}{l} G_1=(11)T_1+(21)T_2+\ \ldots\ +(n1)T_n \\ G_2=(12)T_1+(22)T_2+\ \ldots\ +(n2)T_n \\ \cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot \\ G_n=(1n)T_1+(2n)T_2+\ \ldots\ +(n_n)T_n \end{array}\right\}\ \ldots\ (\text{II}).$$

Apply and keep applied to each of the bodies, $B_1, B_2 \ldots B_n$ (in practice by the weights of the pulleys, and by counter-pulling springs), such forces as shall have for their moments the values $G_1, G_2 \ldots G_n$, calculated from equations (II) with whatever values seem desirable for the tensions $T_1, T_2 \ldots T_n$. (In practice, the straight parts of the cords are to be approximately vertical, and the bodies B_1, B_2, are to be each balanced on its axis when the pulleys belonging to it are

* The idea of force here first introduced is not essential, indeed is not technically admissible to the purely kinematic and algebraic part of the subject proposed. But it is not merely an ideal kinematic construction of the algebraic problem that is intended; and the design of a kinematic machine, for success in practice, essentially involves dynamical considerations. In the present case some of the most important of the purely algebraic questions concerned are very interestingly illustrated by these dynamical considerations.

removed, and it is advisable to make the tensions each equal to half the weight of one of the pulleys with its adjustable frame.) The machine is now ready for use. To use it, pull the cords simultaneously or successively till lengths equal to $e_1, e_2, \ldots e_n$ are passed through the rings $E_1, E_2, \ldots E_n$, respectively.

The *pulls* required to do this may be positive or negative; in practice, they will be infinitesimal, downward or upward pressures applied by hand to the stretching weights which (§) remain permanently hanging on the cords.

Observe the angles through which the bodies $B_1, B_2, \ldots B_n$ are turned by this given movement of the cords. These angles are the required values of the unknown $x_1, x_2, \ldots x_n$, satisfying the simultaneous equations (I).

The actual construction of a practically useful machine for calculating as many as eight or ten or more of unknowns from the same number of linear equations does not promise to be either difficult or over-elaborate. A fair approximation being found by a first application of the machine, a very moderate amount of straightforward arithmetical work (aided very advantageously by Crelle's multiplication tables) suffices to calculate the residual errors, and allow the machines (with the setting of the pulleys unchanged) to be re-applied to calculate the corrections (which may be treated decimally, for convenience): thus, 100 times the amount of the correction on each of the original unknowns, to be made the new unknowns, if the magnitudes thus falling to be dealt with are convenient for the machine. There is, of course, no limit to the accuracy thus obtainable by successive approximations. The exceeding easiness of each application of the machine promises well for its real usefulness, whether for cases in which a single application suffices, or for others in which the requisite accuracy is reached after two, three, or more of successive approximations.

December 12, 1878.

W. SPOTTISWOODE, M.A., D.C.L., President, in the Chair.

Dr. Philipp Hermann Sprengel was admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Flow of Water in Uniform *Régime* in Rivers and other Open Channels." By JAMES THOMSON, LL.D., D.Sc., F.R.S., and F.R.S.E., Professor of Civil Engineering and Mechanics in the University of Glasgow. Received August 15, 1878.

In respect to the mode of flow of water in rivers, a supposition which has been very perplexing in attempts to form a rational theory for its explanation, has during many years past, during at least a great part of the present century, been put forward as a result from experimental observations on the flow of water in various rivers, and in artificially constructed channels. It was, I presume, put forward in the earlier times only as a vague and doubtful supposition; but, in later times it has, in virtue of more numerous and more elaborately conducted experimental observations, advanced to the rank of a confirmed supposition, or even of an experimentally established fact. This experimentally derived and gradually growing supposition was perplexing, because it was in conflict with a very generally adopted theory of the flow of water in rivers which appeared to be well founded and well reasoned out.

That commonly received theory, which for brevity we may call the *laminar theory*, was one in which the frictional resistance applied by the bottom or bed of the river against the forward motion of the water was recognized as the main or the only important drag hindering the water, in its downhill course under the influence of gravity, from advancing with a continually increasing velocity; and in which it was assumed that if the entire current is imagined as divided into numerous layers approximately horizontal across the stream, or else trough-shaped so as to have a general conformity with the bed of the river, each of these layers should be imagined as flowing forward quicker than the one next below it, with such a differential motion as would generate through fluid friction or viscosity, or perhaps jointly with that, also through some slight commingling of the waters of contiguous layers, the tangential drag which would just suffice to prevent further acceleration of any layer relatively to the one next below it. Under this prevailing view it came to be supposed that for points at various depths along any vertical line imagined as extending from the surface of a river to the bottom, the velocity of the water passing that line would diminish for every portion of the descent from the surface to the bottom.

The experimentally derived and perplexing supposition for which no tenable theory appears to have been proposed, though the want of such a theory has been extensively felt as leaving the science of the flow of water in rivers in a state of general bewilderment, is, that inconsistently with the imagination of the water's motion conceived

under the laminar theory, *the forward velocity of the water in rivers is, in actual fact, sometimes or usually not greatest at the surface with gradual abatement from the surface to the bottom;* but that when the different forward velocities are compared which are met with at successive points along a vertical line traversing the water from the surface to the bottom, it may often be found that the velocity increases with descent from the surface downwards through some part of the whole depth, until a place of maximum velocity is reached, beyond which the velocity diminishes with further descent towards the resisting bottom.

That the superficial stratum of water flowing downhill under the influence of the earth's attraction should not have its forward velocity continually accelerated until, by its moving quicker than the bed of water on which it lies, a frictional drag would be communicated to it from below, by that supporting bed of water, sufficient to hold it back against further acceleration, has appeared very paradoxical. In various cases, during a long period of time, the alleged result appeared so incredible that the experimental evidence was doubted, or was dismissed as untrustworthy. In some cases the phenomenon was admitted as a fact, but was attributed to a frictional drag or resistance applied to the surface of the water by the superincumbent air, even in case of the air being at rest with the water flowing below, or more strongly so when the wind might be blowing contrary to the motion of the river.

Omitting to touch on the experimental results, and the opinions of various investigators in the older times, as I have not had sufficient opportunity to scrutinise them in detail, I have to refer to the investigations conducted at about the year 1850 by Ellet on the Mississippi and Ohio Rivers.* He was led to the conclusion† from his own experiments on the Mississippi, that the mean velocity of that river (or at least the mean velocity of the great body of its current, as the part near the bottom or bed of the river had not been definitely included in his researches) instead of being less, is in fact greater than the mean surface velocity. He attributed this phenomenon, which he regarded as indubitably proved, and which if true must certainly be very remarkable, to a frictional drag or resistance, against the forward motion, applied to the surface of the water by the atmosphere in contact with the surface. Like suppositions had previously been made by some observers and theoretical investigators in Europe, as may be gathered from D'Aubuisson "Traité d'Hydraulique," 2nd edition, 1840, p. 176, and from other sources of information.

* Ellet on the "Mississippi and Ohio Rivers." Philadelphia: 1853. This is a republished edition of a Report to the American War Department by Ellet on his investigations, which were made under authority of an Act of Congress.

† Pages 37 and 38 of the book referred to in the preceding note.

Other experimental researches on the flow of the Mississippi River, much more elaborate than those of Ellet, were made in the period between 1850 and 1861 by Captain Humphreys and Lieutenant Abbot, with others acting under authority from the American Government, and an account of them was published as a Report by Humphreys and Abbot in 1861.* These experiments and the investigations exhibited in the report, where the observed results are combined in various ways so as to bring out average results and more or less probable conclusions for various circumstances, lead very clearly and very convincingly to the conclusion that ordinarily the maximum velocity is not at the surface but at some depth below it, usually much nearer to the surface than to the bottom, and often at some such depth from the surface as $\frac{1}{4}$ or $\frac{1}{3}$ of the whole depth of the water. These investigators (Humphreys and Abbot) show further (at pages 285, 288, and 289 of their Report) that this phenomenon is not wholly nor even mainly due to any frictional resistance applied by the superincumbent atmosphere to the forward flow of the surface of the water; because they found that even when the wind is blowing in the direction of the river current, and advancing at the same velocity as that current, so that the air lies on the surface of the water without relative motion, the phenomenon manifests itself almost in as great a degree as when the air is lying at rest relatively to the land; and found yet further that the phenomenon still manifests itself even when the wind is blowing in the direction of the flow of the river much faster than the current, so that it blows the water surface forward instead of applying a resisting drag or backward force to the surface.

At about the middle of the present century very important experiments on flowing water were made in France by Boileau, and by Darcy and Bazin; and elaborate accounts of these researches were published.†

The experiments comprised among the researches of Boileau and of Darcy and Bazin, to which I have to refer as bearing on the special subject of the present paper, relate to the flow of water in long channels and conduits constructed artificially, some in wood and some in masonry and other materials. The channels or conduits in different cases were of widths comprised between half a metre and two metres. In some of the more important experiments the channels were con-

* Report on the "Physics and Hydraulics of the Mississippi River." By Captain A. A. Humphreys and Lieutenant H. L. Abbot. Philadelphia: 1861.

† Boileau: "Traité de la mesure des eaux courantes." Paris: 1854. Darcy: "Recherches experimentales relatives au mouvement de l'eau dans les tuyaux." Paris: 1857. Darcy et Bazin: "Recherches Hydrauliques." Paris: 1865. This last book constitutes a memoir by Bazin on researches commenced by Darcy, and continued for some time by him with the aid of Bazin; and, after the death of Darcy in 1858, continued by Bazin, and by him completed and worked out in the discussion of their results.

structed in wood, and were open above, and had a flat bottom and vertical sides, so that the current was rectangular in cross-section. Channels of various other forms were also used, and the mode of flow of the water in them was scrutinized. The results arrived at by these experimenters tend very much towards establishing the supposition which forms the subject of the present paper—the supposition namely of the prevalence or frequent occurrence of a distribution of velocities having the maximum velocity not at the surface but at some moderate depth below. Boileau, by his experiments, was led to announce as one of his conclusions (page 308), that in the medial longitudinal vertical section of a rectangular canal with uniform *régime*, the maximum of velocity is situated not at the surface, but at a depth which is a fraction more or less considerable of the total depth of the current. He also announced, as a conclusion, that the decrease of velocity, from the place of maximum velocity up to the surface, must be attributed to some new cause different from that which produces the diminution of velocity from the place of maximum down to the bottom. This new cause, he says, cannot be solely the resistance of the bed of air in contact with the liquid surface acting like the face of a pipe or conduit; and he assigns, in proof of this, the reason that the mobility of this bed of air does not permit of our attributing to it a retarding influence so great as that which is implied in the rapid abatement of velocities in approach towards the surface in the upper part of the current. He recounts his own special experiments, made in 1845, on the influence of wind on the velocities in currents,—a subject which he says had up to that time been very little investigated by hydraulicians. He deduces from his experiments conclusions (page 313) to the effect that in spite of varied disturbances produced by wind blowing over the water with varied intensity, yet there is manifested a very sensible tendency to a decrease of velocities of the water for approach towards the liquid surface; and that the maximum velocity is yet below the surface, even when the wind blows forward with the current, and has a velocity greater than that of the current. Judging, then, that resistance of the air cannot be the cause of the phenomenon, he says that it is then principally in the mutual actions which bind among one another the liquid particles, and in the oblique and rotatory movements which result, under the influence of these forces, from the difference of velocities of neighbouring particles, that it is necessary to seek for the explanation of the phenomena of the decrease of velocities in the approach towards the surface of currents. He goes on to say that we have to conceive, in fine, that these oblique movements, producing transverse living forces ("*forces vives*"), diminish according to certain general laws, the living forces of forward motion which the hydrometric instruments are adapted to indicate.

I have cited this passage from Boileau very fully, because it seems

to me to contain the nearest approach towards an explanation of the phenomenon in question of any that have been attempted, so far as any such attempts have come under my notice. It involves, I think, at least a glimmering towards a true explanation; but I regard it as being in great part erroneous, and importantly so in principle, and as being besides altogether incomplete. I do not think it has been offered by the very able investigator himself, who has proposed it, as being at all sufficient; but I think it has been offered only as tending to throw some light over the region for further search, and some indication towards courses in which speculation and research might well advance.

Bazin's experiments, of the general character already mentioned, were very extensive in their scope, and were carried out in great detail, and with some remarkable refinements of method. The velocities were measured mainly or wholly by a modification devised by Darcy of the well known instrument called Pitot's tube. Bazin, in the case of canals not very wide relatively to the depth of the current, found very clearly and decisively the phenomenon in question of the maximum velocity being below the surface. But, in the case of rectangular channels of more considerable width, channels having the width of the current so much as four or five times the depth or more, Bazin by his scrutiny and consideration of his experimental results, was led to conclude that the diminution of velocity for approach towards the surface in the upper part of the current is to be found only in the side parts of the current—the parts flowing along the two side walls. He judged that throughout the whole of the current, except two side parts, each having some moderate width, which might be equal to about twice the depth of the current, the maximum of the velocities for all points, situated in a vertical line, is to be found at the surface; and that the rate of diminution of velocity for descent from the surface would begin as nothing at the surface, and would go on increasing with descent to the bottom. His experiments, according to his own careful analysis and combination of them, appeared to be in agreement with this assumption, or to bring this supposition out as a result.

I do not, however, regard this conclusion as being trustworthy. His experiments for the case of great width relatively to depth had not, in any instance, a depth of water exceeding ·38 of a metre, or $1\frac{1}{4}$ foot, and thus the depths were so small absolutely as not to admit of a fine enough discrimination of minute changes of velocity for minute changes of depth of the point where the velocity was observed, nor of measuring velocities close enough to the surface. So far as experimental researches go, some doubt I presume must still remain over this part of the subject. Indeed, the Indian experiments, next to be mentioned, show results in disagreement with this conclusion offered by Darcy.

Quite recently, in 1874–75, experiments were conducted in India on the Ganges Canal, close to Roorkee, by Captain Allan Cunningham, R.E.* These experiments bring out among their results, very remarkably, the frequently alleged phenomenon of the maximum velocity of the water being not at the surface, but at some moderate depth below. And further, it is deserving of special notice that those of his experiments, which have chiefly to be referred to as throwing light on this subject, were made in an aqueduct about 85 feet wide, and with an approximately level bottom; and that the depths of the water in different experiments ranged from about 6 feet to about 9½ feet, so that the width was on different occasions from about nine times to about fourteen times the depth, and yet the maximum of the velocities at mid-channel (or the maximum velocity in the longitudinal medial vertical section) came out by averages of numerous results, and, by varied modes of experimenting, to be very decidedly below the surface.

Experiments carried out lately on a very large scale on the Irawaddy river by Robert Gordon, Executive Engineer, British Burmah, Public Works Department, go to confirm the truth of the same phenomenon. These experiments of Mr. Gordon, however, although valuable in many respects, appear to be subject to some doubt as to whether, through the mode of experimenting, the level of supposed maximum velocity has not been brought out too low, that is to say, too near the bottom. On this point Mr. Gordon (in his Introductory Note, § vii, page ii of date 16th June, 1875) intimates his intention to make further experiments with other instruments, but still asserts his confidence in his previous methods and results.

Until about two years ago I had not happened to become acquainted with any of the evidence for the phenomenon in question except the unsatisfying experimental results given by Ellet; but about two years ago I met with accounts of some of the more recent and more convincing experimental investigations. It then appeared to me that if the asserted phenomenon must really be accepted as a truth, there ought to be some mode possible of accounting for it: and a theory occurred to me which I now propose to submit.

The mode of thought which near the beginning of the present paper I have described as constituting the laminar theory, I must premise, has long appeared to me to be an erroneous and a very misleading view. It was a very prevalent mode of thought, and was usually too influential on people's minds even when they did entertain decidedly, though often not clearly enough, the consideration of eddies and transverse movements or commingling currents with different velocities.

* "Hydraulic Experiments at Roorkee, 1874–75," by Captain Allan Cunningham, R.E., published in "Professional Papers on Indian Engineering." Thomason, College Press, Roorkee, 1875: also Spon and Co., London, &c.

The great distinction between the mode of flow of a very viscid fluid, such as treacle or tar, and the mode of flow of water in ordinary circumstances in pipes and in open channels, has not been enough generally and enough consistently attended to. The laminar theory constitutes a very good representation of the viscid mode of motion; but it offers a very fallacious view of the motion in the flow of water in ordinary cases in which the inertia of the various parts of the fluid is not subordinated to the restraints of viscosity.

In the flow of water in an open channel in ordinary circumstances the earth's attraction is perpetually tending to accelerate the forward motion of the water throughout the whole body of the current in consequence of the surface declivity; or we may say, with more complete expression, in consequence of the fall of *free-level** which, in virtue of the surface declivity, occurs to all particles in the current as they advance in their down-stream course. The tendency to increase of velocity, if we neglect the backward or forward force, usually very small, or it may be nothing, applied by the air to the water surface, we may say is counteracted solely by a backward resisting force-system applied by the wetted face of the channel to the water momentarily in contact with it. The wetted channel face, it must be observed, is ordinarily more or less rough with gravel, mud, weeds, or other asperities. It is not a true view to imagine a smooth channel face washed by a thin lamina of water, which imagined lamina of water receives a backward or resisting force-system applied tangentially by the so imagined channel face, and transmits tangential backward force to another lamina of water lying next to itself on the side remote from the channel face. It is not the case that from any layer of water whatever, thick or thin, spread over the channel face, resisting forces are transmitted to the interior of the body of the current in any great degree by mere viscid resistance to change of form in the intervening fluid, as would be the case if it were like treacle or tar. But, very differently, indefinite increase of velocity of the water situated in the interior of the current is prevented by continual transverse flows thereto, and commingling therewith, of portions of water already retarded through their having been lately in close proximity to the resisting channel face; and, jointly with that, by the condition that portions of the fluid which have been flowing forward temporarily in

* The *free-level* for any particle of water, in a mass of statical or of flowing water, is the level of the atmospheric end of a column, or of any bar of statical water, straight or curved, having one end situated at the level of the particle, and having at that end the same pressure as the particle has, and having the other end consisting of a level surface of water freely exposed to the atmosphere, or else having otherwise atmospheric pressure there. Or, briefly, we may say that the *free-level* for any particle of water is the level of the atmospheric end of its pressure-column, or of an equivalent ideal pressure-column.

the interior of the current, and have been gaining forward acceleration there are gradually expelled, or do gradually flow from that region, and come themselves into close proximity to the resisting channel face; and so, in their turn, do receive very directly backward forces from the face, because in proximity to it processes of fluid distortion subject to viscid resistance are going on with great activity and intensity.

The transverse motions have their origin primarily in the rush of the water along the wetted channel face. When that face is rough or irregular with lumps and hollows or other asperities, reasons for the origination of transverse currents may be sufficiently obvious. But even if the channel face is extremely smooth, so as to present no sensible asperities, still there is good reason to assert that transverse flows will come to be instituted in consequence of the rapid flow of the main body of the current along a lamina, very thin it may be, of water greatly deadened as to forward motion by viscid cohesion with the channel face, and throughout and across which, if regarded as only very thin, in virtue of its thinness, the backward force applied by the face can be transmitted by mere viscosity. The thin lamina of deadened water will tend by the scour of the quicker going water always moving subject to variations both of velocity and of direction of motion to be driven into irregularly distributed masses; and these, acted on by the quicker moving water scouring past them, will force that water sidewise, and will be entangled with it and will pass away with some transverse motion to commingle with other parts of the current.*

If we watch the surfaces of flowing rivers, or of tidal currents flowing in narrows or *kyles*, we may often have opportunity to observe very prevalent indications of rushes of water coming up to the surface and spreading out there. These rushes often may be seen to keep rising in quick succession in numerous neighbouring parts of the

* This principle I noticed myself in the connexion in which it is here adduced; and the idea has since been confirmed to me and rendered more definite through additional considerations mentioned to me lately by my brother, Sir William Thomson, which have originated with him in some of his theoretical investigations in quite another branch of hydraulic science, and which relate to finite slip in a frictionless fluid. He pointed out that if, for water theoretically regarded as frictionless, or devoid of viscosity, we imagine a long smoothly formed straight trough or channel with a thin vertical longitudinal plane septum dividing it into two parts each uniform in cross-section throughout its length, and if we imagine the space on one side of the septum to be occupied by still water, and a current to be flowing along on the other side; and if, while this is in progress, we imagine the vertical partition to be withdrawn so as to leave the current flowing along a plane face of still water, the motion with the finite slip thus instituted will be essentially unstable. Reasons for this, when once it is brought under notice, are very obvious from consideration of the centrifugal forces, or centrifugal actions, which would be introduced on the slightest beginning being made of any protuberance or hollow in the originally plane interface between the still water and the current.

water surface, and they may be seen presenting appearances of spreading out till they meet one another and give indication of momentary downward sinking at their places of meeting.

From whence do these transverse currents come to the surface? It seems to me they must have had their origin in the deadened water scouring along the bottom, or along the wetted side-faces of the channel, in such ways as have just now been briefly sketched out. Thus it seems that there are tendencies bringing about the result that the superficial stratum of the river receives perpetually renewals of its substance by water currents arriving to it, and spreading out there, which have very recently departed from the bottom before coming up to enter into that superficial stratum. But their substance, having come in great part from the bottom, must be largely made up of the deadened or slow-going bottom-water. It is to be understood that this deadened water, in rising through the current towards the surface, is partly urged forward in the down-stream direction by the surrounding quicker-going water, but that it arrives at the surface without having attained fully to the down-stream velocity of that intermediate stream.

It may readily be perceived that it is from the washed face of the channel alone, or from that and the retarded layer of water in proximity to it, that any strong transverse impulses can be applied to any parts of the current. No rapid transverse current will originate in the middle of the body of the river; for there is no cause for the origination of transverse currents there, unless perhaps we were to regard as such any slight transverse motions which may be produced through the gliding forward of parts of the water there relatively to others near them going with different velocities, and unless we were to regard as such any transverse disturbances that may be imparted to forward-flowing water there by the intrusion and commingling of partially deadened water from the channel-face.

We may now have great confidence, I think, in taking as a well-established truth, or at least as a very probable view, the supposition already laid down to the effect that very commonly the superficial stratum of a river receives perpetually renewals of its substance by water currents arriving to it and spreading out there, which have very recently departed from the bottom or sides of the channel before coming up to enter into that superficial stratum; and that the substance thus perpetually renewing the surface stratum is largely composed of deadened or slow-going bottom-water, or of water going slower forward than the water through which it traverses in ascending to the surface. It is further to be noticed that the water which at any moment constitutes the superficial stratum is, in its turn, very soon overflowed by later arrivals from the bottom. So it gradually descends from the surface into the interior of the body of the river. But during this action it is always flowing downhill, or we may better say it is experiencing

a fall of free level, in consequence of the surface declivity. It is thus receiving forward acceleration in the downhill direction, and its velocity goes on increasing until at some depth from the surface it reaches a maximum, from whence, during further lapse of time and further descent of this water towards the bottom, the retarding influences imparted to it from the bottom are predominant over the downhill accelerating influence of gravity. These retarding influences, chiefly acting through transverse rushes of water from the bottom commingling more numerously and more briskly with the descending water under consideration the more it gets into the neighbourhood of the bottom, bring about the result that the water goes forward with less and less velocity as it approaches nearer and nearer to the bottom.

I have now to offer, by consideration of an imaginable case different from that of an ordinary river, an illustration which will aid in the forming of clear ideas on what I have been presenting as a true theory of the real behaviour of the water in rivers.

Let us imagine a flowing river composed mostly of water, but with a layer of oil floating on the top, the oil being of some such depth as a tenth or a twentieth part of the whole depth of the river. Let us suppose the width of the river to be so very great relatively to the depth as that in considering the flow in a middle portion of the river, we may regard it as experiencing no sensible retarding influences, either through the water or the oil, from the sides of the river; and let the flow to be kept under consideration be only that middle portion without the lateral portions which would be sensibly affected by retarding influences from the sides. Here we have a case differing from that of an ordinary river of water in this important respect, that, while in the ordinary river the superficial stratum of fluid is perpetually changing its substance, and is, as I suppose, perpetually receiving new supplies of deadened water from the bottom, in the imagined case now adduced the substance of the superficial layer being of oil floating at top, does not undergo any such change. The oil then, it seems very certain, would really rush down what we may call the inclined plane of water on which it lies, and would go on accelerating its motion until, by advancing very much faster than the water, it would introduce a frictional drag between itself and the water sufficient to hinder its further acceleration;* or rather until, without attaining to

* *Postscript note, 1st November*, 1878.—An observed phenomenon, which, if duly taken into consideration, must doubtless be found to be closely allied in its nature to the supposed behaviour of the imagined layer of oil on a flowing river of water above adduced, and which is certainly of much interest, both for its own sake and in reference to theoretical views which have been held as to its origin and its indications, has come under my notice since the time when the present paper in manuscript was presented to the Royal Society. The book by Bazin, which may be briefly named as Darcy et Bazin "Recherches Hydrauliques," Paris, 1865 (*see* a previous foot-note in this paper), contains prefixed to it a report, dated 1863, of a committee of the

that stage of great relative velocity, it would at an earlier stage ruffle up the mutual face of meeting of itself and the water into protuberances and hollows, somewhat like waves, on the principle referred to already in a foot-note as having been proposed by Sir William Thomson, and would carry this action on to the extent of causing commotion and commingling of the water and oil. The contrast between this case and that of an ordinary river of water is so remarkable as to aid the forming of a clear comprehension of the very different mode of action which I have been attributing to the water in ordinary rivers and other open channels.

It is further worthy of notice that if, from any local cause, the water flowing forward in some part of the width of a river has in its motion a component downward from the surface towards the bottom, and is free from intrusion of upward currents or rushes of deadened water

Academy of Sciences on the memoir of M. Bazin, "Sur le Mouvement de l'Eau dans les Canaux decouverts." In that report the committee remark (as confirmatory of the view which they accept, to the effect that in deep rivers, especially when not very wide relatively to their depth, the place of maximum velocity is at a considerable depth below the surface) as follows :—" Il y a longtemps que les bateliers du Rhin et nos pontonniers savent qu'un bateau chargé et ayant un fort tirant d'eau, marche, en descendant, plus vite que l'eau qui le soutient ou que les corps flottants à la surface." This obviously conveys the opinion that a heavily loaded boat, sinking deep into the water, and thereby having its deeper part immersed in water which is flowing quicker than the surface water, is dragged forwards by that deeper and quicker moving water, and so is made to advance quicker than the surface water does. The idea seems to be that the boat has some average velocity less than that of the water at its bottom, and greater than that of the surface water. The view which thus appears to be held in respect to the observed phenomenon seems to me to be inadequate and erroneous. On the principle put forward above in the present paper in reference to the imagined case of a river with an upper layer of oil, I would suppose that a large and heavy boat, even if flat-bottomed and of shallow draught of water, would run down the river-course quicker than the water in which it swims; for the reason that while all the water surrounding it makes occasional visits to the bottom of the river, and meets with great retardation there, the boat does not dive to the bottom, and is free from any such retardation, and so is only held back by the surrounding water against taking from gravity a perpetually increasing velocity. Thus it must go faster than the surrounding water which has to hold it back. The boat of deeper draught referred to by the committee I would suppose would advance quicker than the surface water, for the same reason, and not merely because of its bottom being situated in water moving quicker than that at the surface. The principles I have assigned would afford ample reason for our supposing that the boat of deep draught might swim forward much quicker not only than the surface water, but also than the water at its bottom, or indeed than any part of the water of the river surrounding the boat. Very small floating objects, such as sticks or leaves, would present, in proportion to their small masses, so much resistance to motion through the surrounding water that they would be constrained in fact to move sensibly at the same velocity as that of the water surrounding them. The phenomenon would thus be presented of the boat swimming forward past the small floating objects around it.

J. T.

from the bottom, or of water retarded by the influence of the river-bed, we ought to expect the forward velocity to increase from the surface to very nearly the bottom. The accelerative influence of gravity due to the surface inclination, and more particularly due to the fall of free-level experienced, as an accompaniment of that inclination, by the water throughout the body of the current in its onward flow would generate in every portion or particle of this water increase of velocity for advance along its course; because, in the absence of rushes of deadened water from the bed, such as it appears do commonly intrude into the body of the current, there would be no retardative influence to counteract the gravitational accelerative influence; since the mere viscosity of the water unaided by transverse commingling is, I consider, insignificantly small and quite ineffectual as a resisting influence or means of transmitting resistance from the bed to any part of the water in the body of the current out of close proximity to the bed. But as this forward moving water is also descending towards the bottom while it is gaining forward velocity, it follows that, in the circumstances of flow supposed, we ought to expect the forward velocity to increase with descent from the surface to very nearly the bottom. It is to be understood that the freedom supposed from upward rushes or intrusions of deadened water will not be maintained in the water when it arrives into proximity to the bottom. In approaching very near to the bottom the water must begin to receive important resisting forces communicated to it from the bottom through commingling of deadened water, and by intense distortional actions with viscosity.

It is also to be noticed in connexion with the case under consideration that if, in one part of the width of the river, there is a prevailing descent towards the bottom, there will be upward flows to compensate for this in other parts of the width. Then obviously the whole character of the action of the water will be very different in the regions where ascent prevails from that in the regions where there is a prevailing descent; and the distribution of forward velocities throughout any vertical line in the one region will be quite different from the distribution of forward velocities throughout any vertical line in the other region. Local circumstances casually affecting the flow in the way here described I think may perhaps account for some of the apparent anomalies in respect to the distribution of velocities through different parts of the depth from surface to bottom which have been met with by various experimenters, and have been included among the recognised causes of the perplexity and bewilderment with which this branch of hydraulic science is pervaded.

I wish next to draw attention to one of the results of observation and experiment announced by Captain Cunningham in his book already referred to ("Hydraulic Experiments at Roorkee"). In his discussion of his experimental results on the flow of water in each of

two artificially-formed channels on the Ganges Canal, one of them, 168 feet wide, and the other 85 feet wide, and each having the water often about from 6 feet to 9 feet deep, he states (p. 46, article 35): "There is a constant surface motion (deviation) from the edges towards the centre, most intense at the edges and rapidly decreasing with distance from the edges."

This experimental conclusion, on the supposition of its being decidedly trustworthy, as Mr. Cunningham asserts with confidence that it is, I think may probably be satisfactorily explicable through considerations intimately connected with those which I have already given for an amended theory of the flow of water in rivers.

I wish, however, not to prolong the present paper by entering on any detailed discussion of this branch of the subject, and besides I prefer to reserve this for some further consideration before venturing to put forward the views in reference to it which at present appear to me likely to be tenable. It may be noticed, however, that Captain Cunningham's experimental result, if decidedly correct, throws additional light on the subject of the abatement of surface velocity comparatively to the velocity at some depth below the surface being found in Bazin's experiments to occur in a much greater degree near the sides of rectangular and various other channels than at middle. Bazin thought indeed from his own experiments (as I have already had occasion to mention) that the relative retardation or slowness of the surface occurred not in the middle of wide channels (that is to say, of channels wide relatively to the depth of the water) but only near the sides; but this supposition I have referred to as appearing not to be trustworthy. With these brief suggestions I will now leave for further consideration the subject of the special phenomena of the influence of the sides.

Historical Note.

Subsequently to my having formed, in all its primary or more essential features, the new view now explained of the flow of water in rivers, and before I had met with the book of Humphreys and Abbot, I happened to see in the writings of another author (paper of Mr. Gordon already referred to) the following remark in reference to their views as to the velocity at the surface being less than at some depth below. "Humphreys and Abbot attribute the fact to transmitted motion from the irregularities of the bottom; but confess themselves dissatisfied with their own explanation."

These words seemed to me to indicate a probability of Humphreys and Abbot having anticipated me in some part at least of the theory which I had been forming. On obtaining their book, however, and reading the passage referred to, not by itself alone, but with its context, it appeared to me that it involved no real anticipation,

although one clause of a sentence in it, read by itself, might be supposed to do so. The passage is to be found in their work at p. 286. They begin by saying, that their experimental observations detailed in their previous pages "prove that even in a perfectly calm day there is a strong resistance to the motion of the water at the surface as well as at the bottom," and that this resistance at the surface "is not wholly or even mainly caused by friction against the air." They go on to say:—"One important cause of this resistance is believed to be the loss of living force, arising from upward currents or transmitted motion occasioned by irregularities at the bottom. This loss is greater at the surface than near it. The experiment of transmitted motion through a series of ivory balls illustrates this effect. It is likewise illustrated on a large scale by the collision of two trains of cars on a railway, in which case it has been observed that the cars at the head of the train are the most injured and thrown the farthest from the track; those at the end of the train are next in order of injury and disturbance; while those in the middle of the train are but little injured or disturbed. Other causes may and probably do exist, but their investigation has, fortunately, more of scientific interest than practical value. For all general purposes it may be assumed that there is a resistance at the surface, of the same order or nature as that which exists at the bottom."

Now although this passage does contain the words "*arising from upward currents or transmitted motion occasioned by irregularities at the bottom,*" yet the illustrations, by means of the series of ivory balls, and of the collision of railway trains, show that the authors attribute to those words no clear and correct meaning, but, on the contrary, I would say they put forward quite a false view of the actions going on. Besides I myself do not admit that, except from the air, there is a resistance at the surface. According to my supposition the already resisted and retarded bottom water comes to the surface and spreads out there, but receives no new resistance there, and on the contrary receives acceleration from gravity in running down hill.

II. "The Magic Mirror of Japan." Part I. By Professors W. E. Ayrton and John Perry, of the Imperial College of Engineering, Japan. Communicated by William Spottiswoode, Esq., M.A., Treas. R.S., &c., &c. Received October 2, 1878.

The Japanese mirror must, from three points of view, attract the notice of foreigners sojourning in that country—its prominence in the temples, the important feature it forms in the limited furniture of a Japanese household, and the wonderful property (which has apparently

created more interest in Europe than it has in Japan) possessed by certain Japanese and Chinese mirrors of apparently reflecting from their polished faces the raised characters on their backs.

It was for this third reason, the interest that such mirrors have long possessed for the student of science, that our attention was drawn to the subject, and it has been in this direction that our inquiry has been chiefly directed. The results of our investigation we propose giving in the present paper, reserving for a subsequent occasion* some remarks on the Japanese mirror as an object of worship, and the position it holds on the toilet table of a Japanese lady.

The mirror of the Far East is too well known to need an elaborate description; suffice it for the present to observe that it is generally more or less convex on the reflecting side, usually made of bronze, polished with a mercury amalgam, and having at its back a gracefully executed raised design, representing birds, flowers, dragons, a geometrical pattern, or some scene in Japanese mythical history. Occasionally there are in addition one or more Chinese characters (signifying long-life, happiness, or some similar idea) of polished metal, in bold relief. To the method of manufacture we shall refer further on, and especially to the mode in which the convexity of the surface is produced; which portion of the manufacture, while playing, as it does, an important part in the magical behaviour of the mirror, is, as far as we are aware, not to be found described in any of the Eastern or Western writings on the subject.

Just before leaving England, in 1873, the attention of one of the authors was directed to the so-called magic property of certain Eastern mirrors by the late Sir Charles Wheatstone, who explained to him that the Japanese had a clever trick of scratching a pattern on the surface of a bronze mirror which, after being polished, showed no traces of the scratches when looked at directly, but which, when used to reflect the sunlight on to a screen, revealed the pattern as a bright image. This opinion appears to have been shared by Sir David Brewster, since he says, in the "Philosophical Magazine" for December, 1832:—

"Like all other conjurors, the artist has contrived to make the observer deceive himself. The stamped figures on the back (of the mirror) are used for this purpose. The spectrum in the luminous area is *not an image of the figures on the back*. The figures are a copy of the picture which the artist *has drawn on the face of the mirror*, and so concealed by polishing that it is invisible in ordinary lights, and can be brought out only in the sun's rays."

As the explanation, therefore, appeared to this one of the authors to be so simple, and at the same time so complete, he practically dismissed the subject from his mind.

* A lecture at the Royal Institution.

However, he was a little astonished to find, during his residence in Japan, that, although the magic mirror was supposed in Europe to be a standard Japanese trick, and although it had been considered by Sir Charles Wheatstone as one of the best proofs of the ingenuity of the workmen of Japan, still that it formed no part of the stock-in-trade of any of the numerous conjurors in this country, and was never exposed for sale in any of the curiosity-shops. He was also still more surprised when, during the visit of the "Challenger," Sir Wyville Thomson and himself were strolling about Tokio, to find that, although they asked at several mirror shops for a mirror that showed the back, a specimen of which Sir Wyville much desired to possess, the shopkeepers seemed not to have the slightest knowledge of what was wanted. At that time the author could not but regard the total apparent ignorance displayed by the Japanese mirror-vendors on this subject as the result of his limited knowledge of the language, and he had then no notion that, in Japan at any rate, the phenomenon was the result of no clever trickery, but arose from the method in which the mirrors were prepared. We have since learnt, however, by diligent inquiry, that, as is the case with many things appertaining to Japan, so with the magic mirror, the people who know least about the subject are the Japanese themselves, and we think this only furnishes another proof that teachers to instruct the Japanese about Japan itself are the greatest *desideratum.*

Our attention was next directed to the subject of the curious property possessed by some Japanese mirrors by a letter from Professor Atkinson, of the Tokio Dai Gaku (the Imperial University), which appeared in "Nature," May 24th, 1877, and in which he says, after referring to the phenomenon of the pattern on the back being apparently reflected when sunlight is allowed to fall on the face:—

"I have since tried several mirrors, as sold in the shops, and in most cases the appearance described has been observed with more or less distinctness.*

"I have been unable to find a satisfactory explanation of this fact, but on considering the mode of manufacture I was led to suppose that the pressure to which the mirror was subjected during polishing, and which is greatest on the parts in relief, was concerned in the production of the figures. On putting this to the test by rubbing the back of the mirror with a blunt-pointed instrument, and permitting the rays of the sun to be reflected from the front surface, a bright line appeared in the image corresponding to the position of the part rubbed. This experiment is quite easy to repeat, a scratch with a knife, or with any other hard body, is sufficient. It would seem as if the pressure upon the back during polishing caused some change in

* Only a *small* percentage, however, of the total number of Japanese mirrors that the authors of this paper have experimented on show the phenomenon clearly.

the reflecting surface corresponding to the raised parts, whereby the amount of light reflected was greater; or supposing that, of the light which falls upon the surface, a part is absorbed and the rest reflected, those parts corresponding to the raised portions on the back are altered by the pressure in such a way that less is absorbed, and therefore a bright image appears."

Professor Atkinson cautiously adds: "This, of course, is not an explanation of the phenomenon, but I put it forward as perhaps indicating the direction in which a true explanation may be looked for."

In vol. i, p. 242, year 1832, of the "Journal of the Asiatic Society of Bengal," Mr. Prinsep gives an account of a Japanese magic mirror which he had seen in Calcutta. He does not appear to have made any direct experiments with this mirror for the purpose of elucidating which of all the possible causes is the real cause of the magic phenomena, but rather he concludes "from analogy that the thin parts or tympanum of the Japanese mirror are slightly convex with reference to the rest of the reflecting surface, which may have been caused either by the ornamental work having been stamped or partially carved with a hammer and chisel on its back; or, which is more probable, that part of the metal was by this stamping rendered in a degree harder than the rest, so that in polishing it was not worn away to the same extent." It does not seem to have occurred to him that Japanese mirrors are *cast* and *not stamped at all.*

In "Nature," June 14th, 1877, Mr. Highley refers to the exhibition of a Japanese mirror by Professor Pepper some years ago at the Polytechnic Institution, London, and to the praiseworthy attempt of an English brass worker, who saw the experiment, and who also was under the false impression that such mirrors were stamped, to solve the problem. "The workman found that taking ordinary brass and stamping upon its surface with any suitable die, not once, but three times in succession, upon exactly the same spot, grinding down and polishing between each act of stamping, a molecular difference was established between the stamped and unstamped parts, so that images of the pattern could be reflected from the finally polished surface, just as with the Japanese specula, though no difference of surface could be detected with the eye."

To people who have not been in China or Japan, and personally studied mirror-making, this idea of stamping seems very plausible, for Sir David Brewster, on p. 113 *et seq.* of his "Letters on Natural Magic," published in 1842, describes fully a method, depending on the molecular change produced by stamping, by means of which the inscriptions on old coins, that have been worn quite smooth, may be deciphered. This method merely consists in heating the coin on a piece of red-hot iron, when the inscription becomes visible from the different rate of oxidation of the part of the coin that has been subjected to great

pressure in stamping from that part that has been subjected to less. But, as already mentioned, all explanations depending on stamping must at the outset be put on one side when studying the behaviour of Japanese mirrors, since casting, and not stamping, is the process employed in their manufacture.

In the "Reader" (a paper now extinct) for February, 1866, Mr. Parnell attempts to explain the phenomenon by an inequality in the surface of the mirror, produced by the thinner portion warping more in cooling than the thicker part where the pattern exists, and he endeavours to experimentally examine this by studying the direct reflection of the globe of a gas-lamp, as seen in the different parts of the mirror. We, as well as Professor Atkinson, have tried to repeat this experiment with some magic mirrors in our possession, but we cannot say that it affords any conclusive evidence regarding the cause of the phenomenon.

It therefore appeared to us a year ago that the subject would repay investigation, an opinion also expressed by Professor Silvanus Thompson, who, in writing from University College, Bristol, to "Nature," during June of 1877, suggested that the Japanese mirrors exhibited at the Loan Collection of Scientific Apparatus in London might, if they showed the phenomenon, be used for such an investigation. And as Professor Atkinson did not propose following up the question himself, he lent us the mirror which he possessed, and cordially agreed with our proposal that we should undertake the investigation. This we have done, and obtained the results which we venture to submit this evening to the Society.

At the commencement of the inquiry we naturally desired to see what had been written on the subject of Japanese mirrors, and this brought to our notice the information regarding mirrors generally in this country, which, as mentioned at the beginning of this paper, will form, we propose, the substance of a subsequent communication. But, of the *magic mirror*, Japanese literature (so far as we have been able to ascertain) makes absolutely no mention.

In "Les Industries Anciennes et Modernes de l'Empire Chinois," published in 1869, by MM. Stanislas Julien and Paul Champion, there is a short article on "Les Miroirs Magiques des Chinois, et leur fabrication," taken from the paper communicated by M. Julien to the French Academy of Sciences. In this he says:—

"Many famous philosophers have for a long time, but without success, endeavoured to find out the true cause of the phenomenon which has caused certain metallic mirrors constructed in China to have acquired the name of *magic mirrors*. Even in the country itself where they are made no European has, up to the present time, been able to obtain either from the manufacturers, or from men of letters, the information, which is so full of interest to us, because the former keep

it a secret when by chance they possess it, and the latter generally ignore the subject altogether. I had found many times in Chinese books details regarding this kind of mirrors, but it was not of a nature to satisfy the very proper curiosity of philosophers, because sometimes the author gave on his own responsibility an explanation that he had guessed at, and sometimes he confessed in good faith that this curious property is the result of an artifice in the manufacture, the monopoly of which certain skilled workmen reserve to themselves. One can easily understand this prudent reticence when we remember that the rare mirrors which show this phenomenon sell from ten to twenty times as dear as the rest."

M. Julien then gives an elaborate description of one of these mirrors in the possession of the Marquis de La Grange. He further remarks that such mirrors are called in Chinese *theou-kouang-kién*, which means literally "mirrors that let the light pass through them," and that this name has arisen from a popular error on the subject. Chin-kouo, a Chinese writer who flourished in the middle of the eleventh century, speaks with admiration about them in his memoirs called *Mong-ki-pi-tân*, book xix, folio 5. The poet Kin-ma has celebrated them in verse; but up to the time of the Mongolian emperors nobody could explain the cause of the wonderful phenomenon. Ou-tseu-hing, who lived between 1260 and 1341 under this dynasty, had the honour of being the first to throw any light on the subject. He says:

"When we turn one of the mirrors with its face to the sun, and allow it to throw a reflection on a wall close by, we see the ornaments or the characters which exist in relief on the back appear clearly. Now the cause of this phenomenon arises from the employment of two kinds of copper of unequal density. If on the back of the mirror a dragon has been produced while casting it in the mould, then an exactly similar dragon is deeply engraved on the face of the disk. Afterwards the deep chisel-cuts are filled up with denser copper, which is incorporated with the body of the mirror, which ought to be of finer copper, by submitting the whole to the action of fire, then the face is planed and prepared, and a thin layer of lead or of tin spread over it.*

"When a beam of sunlight is allowed to fall on a polished mirror prepared in this way, and the image is reflected on a wall, bright and dark tints are distinctly seen, the former produced by the purer copper, and the latter by the parts in which the denser copper is inlaid."

If, then, we understand this description of Ou-tseu-hing correctly, it would appear that the pattern appears by reflection as a dark image on a bright ground, the opposite of what is experienced in Japanese mirrors.

* This probably refers to the mercury amalgam which is used in polishing, and which Ou-tseu-hing mistook for lead or tin.

Ou-tseu-hing adds that he has seen a mirror of this kind broken into pieces, and that he has thus ascertained for himself the truth of this explanation.

In a recent interesting article published in No. 29 of the "Gartenlaube," Heft 8, 1877, by the well-known German popular writer whose *nom-de-plume* is Carus Sterne, doubt is thrown on the above explanation, since Herr Sterne thinks the magic mirror he himself possesses is too thin for any such inlaying to have been performed. In quoting the information given by M. Julien, to which reference is made above, he incidentally mentions that it is taken from the fifty-sixth volume of the Chinese encyclopædia called "*Ke-chi-king-youen.*" Herr Sterne adds that these magic mirrors were known to the Chinese from the earliest times, and that one of their writers spoke about them in the ninth century of the Christian era. He remarks that the Roman writer Aulus Gellius, who lived seventeen centuries ago, referred to mirrors that sometimes reflected their backs and sometimes did not. From the great antiquity of the Chinese magic mirrors Herr Sterne thinks it probable that the mirrors with secret signs and figures of imps on the back which formed a portion of the stock-in-trade of the witches of the middle ages were of Eastern manufacture. He further alludes to the account given by the Italian historian Muratori of the magic mirror found under the pillow of the Bishop of Verona, who was afterwards condemned to death by Martin della Scala, as well as to the one discovered in the house of Cola da Rienzi, on the back of which was the word "Fiorone."

Neither in "Les Memoires concernant les Chinois par les Missionaires," nor in Duhalde's classical work on China, is there any mention of the magic mirror. I understand, however, that a short paper on the subject, by Professor Harting, appeared some years ago in a Dutch periodical, the "Album der Naturer;" this I have not seen: but Dr. Geerts, a Dutch gentleman resident in Japan, and who has a most extensive acquaintance with the literature bearing on that country, informs me no explanation of the phenomenon was contained in that article.

Japanese literature, as already mentioned, appears to be quite barren of information regarding their own or the Chinese mirrors which appear to reflect their backs. But in the *shim-pen-kamakura-shi*, or "New Collection of Writings about Kamakura," it is mentioned that in the temple *Kenchoji*, situated in the ancient capital of the Shogun,* there is treasured up a wonderful old mirror, 3½ sūns high and 3 sūns wide,† which, when looked at somewhat obliquely, shows the image of

* Shogun, the military usurper of the throne of Japan, and recognized in modern times prior to the revolution of 1869 as the rightful sovereign. He was sometimes erroneously called the Tycoon.

† A *sūn* is nearly one and one-fifth of an inch.

a Buddhist god. This appearance, however, is in no way connected with the pattern at the back, which consists of a new moon reflected in the sea; the artistic balance of the picture being maintained by a rosary and a plum tree. The hole in the upper portion of the mirror is probably for the attachment of a silk cord to hang it up by. The supposed marvellous character of this mirror causes great reverence to be shown to the god of the temple, as it is considered to furnish an undoubted proof of his supernatural character; in fact, the mirror receives nearly as much respect as this Buddhist deity himself.

The way in which the optical effect has been produced is said to be the same as that described in the *Kokon-i-to*, "The Genealogy of the Old and New Physicians," and which is as follows:—Take ten parts of shio (gamboge), one of funso, and one of hosha (borax). Powder these thoroughly, and mix them to the consistency of a paste with a little dilute glue. If any pattern be drawn on the surface of the mirror with this paste, and then allowed to dry, the pattern will be seen, even after polishing, if looked at obliquely.

A mirror, in the face of which was seen the appearance of the famous priest, Shinran-sho-nin, who instituted the Shinshiu religion, to which the Honguangi temples at Kioto belong, was formerly in the possession of the Kuge* Rokujo, and was, to a certain extent, worshipped. Wood-cuts of this mirror were also sold at this nobleman's house, and were regarded as a faithful representation of the priest Shinran-sho-nin. One of the persons formerly employed at the Honguangi temples, Kioto, tells us he remembers, some years ago, a messenger, coming from Mr. Rokujo, asking that the authorities of the temple would give a certificate, stating that the mirror had been constructed by Shinran-sho-nin himself for holy purposes. This, however, they declined to do, believing rather that Mr. Rokujo had fabricated it himself to obtain money on exhibition. Mr. Rokujo, to whom we have applied on the subject, says, that the old tradition in his family was that the mirror originally came from Echigo;† also that, after the failure to obtain a certificate of its sanctity referred to above, he sold it to a temple situated near Kioto, from which, however, it was subsequently removed, and that he is quite unacquainted with its present whereabouts.

A Tokio‡ mirror maker, however, tells us that he has seen an exactly similar mirror at Okasaki-mura, a small village near Kioto, so perhaps this is the present habitation of Mr. Rokujo's old mirror.

It does not appear that this chemical method of preparing the face

* "Kuge," a nobleman formerly attached to the Micado's Court at Kioto, the ancient capital.

† Echigo, a province in the centre of Japan.

‡ "The Eastern Capital," the name given to Yedo since the revolution of 1869, when the Micado transferred his court there from Kioto.

has ever been employed in Japan to alter a portion of the surface in such a way that this part becomes visible in the image formed by reflection, although invisible when looked at directly. A certain Tokio mirror maker, however, said that he had employed the chemical method for this purpose in the following way:—

Coat the surface of the mirror with *urushi* (Japanese varnish), with the exception of the portion that it is desired shall cast the brighter reflection, then act on this part with a paste composed of equal parts of sulphur and copper sulphate, powdered and mixed with *shiro-umedzu* (white plum acid). If this paste, after being allowed to dry on the mirror, which takes about two days, be rubbed off, and the mirror be frequently polished, the pattern (so said this mirror maker) will become invisible when looked at, but will appear in the reflection of the mirror thrown on to a screen. If the above be true, then, if a pattern be drawn on the face of the mirror with the varnish while the remainder of the face is acted on chemically, this pattern should, on reflection, appear darker than the rest. We therefore instructed him to prepare two mirrors, and on the face of one to act chemically on a portion corresponding with the letter "C," while, with respect to the other, he was to leave untouched only a small part of the face, corresponding with the letter "N." This he did; after several polishings of the two mirrors both letters could be seen, either directly or on reflection; after many polishings, however, the letter "C" disappeared for direct vision, but it also disappeared for reflection, and the letter "N" remained visible, either if looked at very obliquely, or when a bright light was reflected on to a screen. In other words, the attempt of this mirror maker turned out a failure. He regards it as resulting from a loss of his former skill, but we are inclined to think that he was confusing the method with which he was acquainted for making an image visible when the face of the mirror is looked at obliquely (the phenomenon which is observed in the mirror at Kamakura), with a method for making the so-called magic mirror, of which he has probably no knowledge. One very interesting fact, however, came out in this experiment, and that was the mirror on which the letter "C" was made, and which did not originally reflect the pattern on the back, acquired the power to do so after *ten* successive polishings. In fact, the mirror maker caused this mirror to acquire the so-called magic character, but in a way unexpected by himself.

Explanations:—The possible explanations of the phenomenon shown by certain Japanese mirrors may be divided into three classes:—

1. The pattern might be scratched on the face of the mirror and hidden by subsequent polishing.

2. The portion of the face corresponding with the pattern might have a different molecular constitution from the metal forming the remainder of the mirror.

This difference in molecular constitution might produce the results:—

a. By causing the portion of the face corresponding with the pattern at the back to attract more mercury, and so to become capable of being polished more easily; or

b. By causing it to be harder, and so to acquire a better polish; or

c. By causing it to polarise light.

This difference in molecular constitution might be produced:—

a. By the inlaying of another metal; or

b. By portions of the surface being acted on chemically; or

c. By unequal density produced by inequality in the rate of cooling; but

d. *Not* by stamping, Japanese mirrors being all cast.

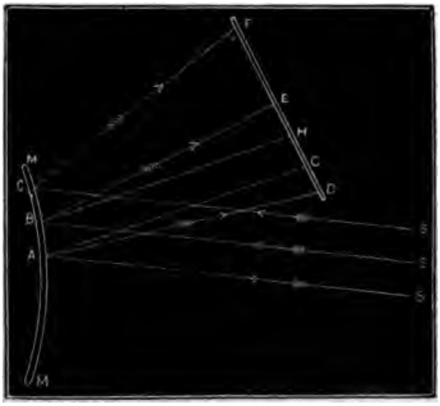
3. The phenomenon might arise from the face of the mirror having intentional or accidental inequalities on its surface, in consequence of which, the part corresponding with the pattern on the back might be relatively concave, and so concentrate the light, or, at any rate, might disperse it less than the remainder of the slightly convex mirror.

The question then resolves itself into considering to which of these three groups of causes is the apparent reflection of the back in some Japanese mirrors due.

To ascertain this, we tried Sir David Brewster's suggestion that the light reflected by the thicker part of the mirror was polarised; but even with a fairly good polariscope, we could detect no marked difference between the light reflected from the various portions of the surface. This failing, we availed ourselves of a very simple method of experimenting, but one that has apparently not suggested itself to previous observers. On one occasion, when some of our students were using lenses to endeavour to make the exhibition of the phenomenon more striking, it occurred to us that the employment of beams of light of different degrees of convergence or divergence would furnish a test for deciding the cause of the whole action. For while, if the phenomenon were due to molecular differences in the surface, the effect would be practically independent of the amount of convergence of the beam of light; on the other hand, if it were due to portions of the reflecting surface being less convex than the remainder, a complete *inversion* of the phenomenon might be expected to occur, if the experiment, instead of being tried in ordinary sunlight, were made under certain conditions in a converging beam—that is, the thicker portions of the mirror might be expected to appear darker instead of brighter than the remainder. Figs. 1—5, which are all much exaggerated for the sake of distinctness, explain this better. MM, fig. 1, represents an ordinary Japanese slightly convex polished bronze mirror. SA, SB, SC, are rays of a parallel beam of light falling on it, and reflected as AD, BE, CF, on to a screen DF; then, if the areas

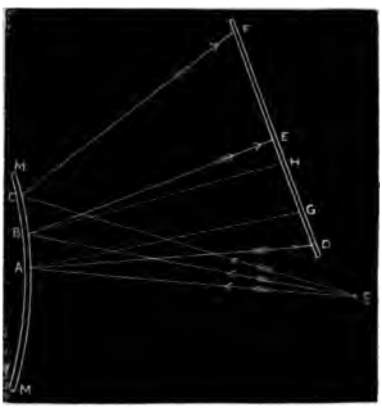
AB and BC of the mirror be about equal to one another, the amount of light falling on them will also be equal; and, since the illuminated areas DE and EF are about equal, they will be equally bright. But if

FIG. 1.



a portion AB of the mirror be, for any reason, flatter than the remainder, then the quantity of light which falls on it, instead of being reflected so as to illuminate the area DE of the screen, will only illuminate some such area as GH. Now, this area being smaller than EF, but receiving the same quantity of light, will appear much brighter than EF; in addition, too, the spaces DG and HE receive but very

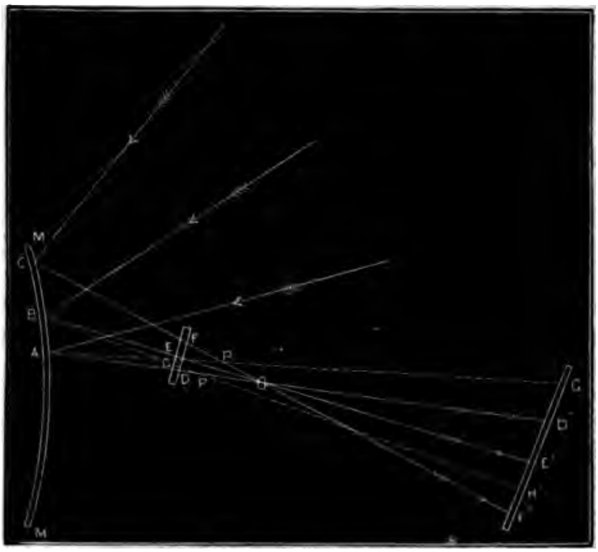
FIG. 2.



little light, and are consequently relatively dark, the excess of brightness, therefore, of the area GH will be apparently much heightened by contrast. And exactly the same reasoning applies to fig. 2, in which the mirror is illuminated by a beam of light diverging from

the point S. But if we now turn to fig. 3, where the light is converging to a point behind the convex surface, and nearer to the surface than half the radius of the mirror, then, after reflection, the light converges to a point O in front of the mirror, and, as before, the area GH (which has become almost a point, G) is smaller, and therefore brighter, than the area DE, as long as the screen is nearer to the mirror than the point P, but larger, and therefore darker, than D'E', when the screen is farther from the mirror than P. In other words, if the phenomenon of the Japanese mirror is due to the curvature of different parts of the surface being slightly different, then with the arrange-

FIG. 3.



ment of light shown in fig. 3, the whole effect ought to undergo an *inversion* as the screen passes through P; that is to say, if the parts corresponding with the pattern at the back are the flatter, then, while these should appear as bright on a dark ground when the screen is at a position DF, they ought to appear as dark on a bright ground when the screen is at a position D'F'. Now this is exactly what is found to be the case when tested experimentally.

Again, if the phenomenon is, as the previous experiment would lead us to conclude, due not to unequal reflecting power of the different portions of the surface of the mirror, but to minute inequalities on the surface, in consequence of which there is more scattering of the rays of light falling on one portion than on another, then since rays of light making very small angles with one another do not separate perceptibly until they have gone some distance, it follows, that if the

screen be held *very near* to the mirror, the apparent reflection of the back, the magical property in fact, ought to become invisible. And this, also, is exactly what happens when we make the screen almost touch the polished surface.

We have, therefore, strong reasons for favouring the "inequality of curvature" theory. In order, however, to make the explanation quite certain, we have had made a small concavity and a small convexity on the face of one of the mirrors, by hammering with a blunt tool, carefully protected with a soft cushion to avoid scratching the polished surface, and, as is seen on trying the experiment, the concavity reflects a bright image and the convexity a dark one when the screen is in the position DF, but when the screen is shifted to D'F', it is the convexity which appears as the bright spot, and the concavity as the dark one.

And not only do we think that the thicker portions of the convex mirror are flatter than the remainder, but the existence of a focus for a divergent pencil (as evidenced by a best position of the screen in fig. 2) leads to the conclusion that, in some instances at any rate, the thicker portion is actually *concave*, and is found to have a radius of about three to four metres.

In the account of the Chinese Magic mirror, given by Ou-tsen-hing at the end of the thirteenth century, he mentions that the wall or screen on which the shadow is cast should be *near*, an instruction which people have usually found it necessary to follow in order to see the phenomenon clearly. But this condition of proximity of the screen to the mirror is necessary, simply because the sunlight falling on the mirror neither forms a parallel beam, nor one diverging from, nor converging to, a single point, but consists, of course, of an enormous number of slightly diverging beams. Consequently, on any *one* point of the mirror there fall rays of light, each making a slightly different angle with the surface. Now, as these, after reflection, proceed in slightly different directions, they will illuminate different points of the screen, and, therefore, make a well-defined image impossible, unless the screen be held near. If ordinary sunlight then be employed, the screen, as previously explained, must be held *not so near* the mirror that the inequalities of the surface are unable to produce any decided displacement of the rays before they strike the screen, and in addition, as we now see, *not so far* from the mirror that the different rays falling on the *same* point are perceptibly separated before they reach the screen; or, putting the above conditions into more precise mathematical language, the screen must not be held so near the mirror that the product of this distance into the angle between the normals to two adjacent parts of the surface is too small, and not so far from the mirror that the product of this distance into the angular diameter of the sun is too large.

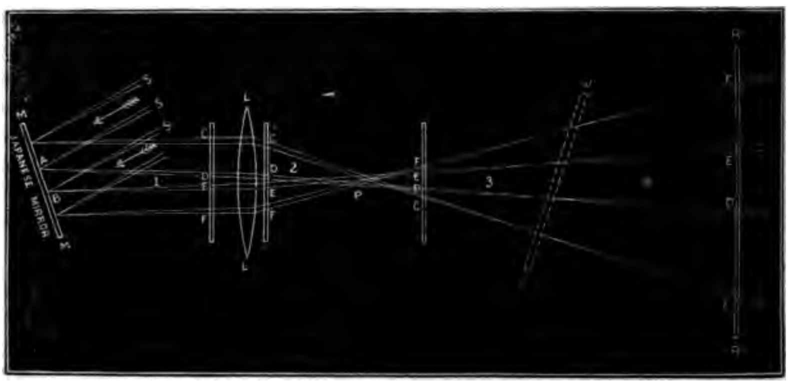
This condition, however, of proximity of the screen to the mirror ceases to have any weight, and the phenomenon can be shown to a large audience by projecting it on a distant wall if one or other of the following devices be adopted :—

1. Allow the sunlight to first pass through a small hole, so that all the rays falling on the same point of the surface of the mirror make the same angle with the surface.

2. Obtain the same result thus :—first let the sunlight fall on a convex lens or on a concave mirror which brings it to a focus, and afterwards causes it to diverge from a single point, then hold the Japanese mirror in the diverging beam at about eight or more feet from the principal focus of the lens or auxiliary concave mirror.

3. Illuminate the mirror with light diverging from a single bright point at some distance, as, for example, from an electric light at the other end of the room, the screen, of course, being shaded from the direct light of the lamp.

Fig. 4.



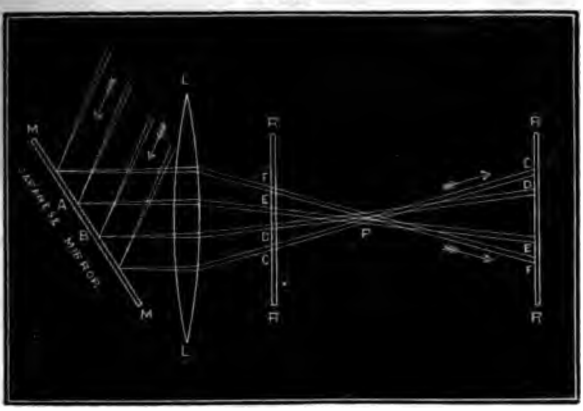
4. Allow the sun's rays to fall on the nearly plane Japanese mirror MM, and after reflection let them pass through a converging lens so adjusted that the screen RR, fig. 4, is beyond the principal focus, P, of the mirror and lens combined, and also beyond JJ, the conjugate focus of the mirror, that is the place at which the image of the Japanese mirror is formed by the lens.

The last method causes the effect to be better than that obtained with ordinary sunlight alone, because the insertion of the lens separates the rays falling on different points of the Japanese mirror more than it separates those which, coming from different points of the sun, are reflected in different directions by the same small portion of the Japanese mirror. In fact, the employment of the lens corrects, to a certain extent, the blurring of the image produced by the sun not being a single luminous point. Number 4 method also economises

the light best, and if the screen is distant, may be employed to produce a large figure of the pattern on the back of the mirror, but the result is not nearly as beautiful as that obtained by either of the former three methods, the first two in particular of which, if the mirror is placed in a darkened room, at about fourteen feet distance from the luminous point produced by a tropical sun, cause the reflection on the wall to assume an appearance startling even to an educated mind, and which might well have brought to the feet of the magician the ignorant poor of the middle ages.

Referring to the arrangement of mirror and lens shown in fig. 4, and remembering the reasoning employed in the case of figs. 1, 2, and 3, we should conclude that if a portion, AB, of the mirror is more concave than the rest, this portion ought to appear as bright on a dark ground if the screen be held in the positions 1, 2, or 4, since, in all these, DE is less than CD or EF, but if it be held at any point, 3 in the region between the principal focus P and JJ, then, since here DE is greater than CD or EF, the concave portion ought to appear as dark on a relatively light ground, while at JJ, the image being uniformly illuminated, the appearance of the pattern ought to disappear altogether. We should expect, then, that the passage of the screen, either through P or through JJ, ought to produce an inversion of the phenomenon if the theory that we are here advocating of the Japanese mirror be correct.

Fig. 5.



Again, imagine the lens LL to gradually move up to the mirror until it attains a very near position, as in fig. 5, then an inspection of the direction of the rays shows that any concave part, AB, of the mirror must appear on the screen as light on a dark ground for all points between the lens and the principal focus P, but that it will be seen as dark on a relatively light ground for all positions of the screen

in the region beyond P. On arranging the light as in fig. 4, and placing the screen successively in the positions 1, 2, 3, JJ, and 4, afterwards moving the lens up to the Japanese mirror, until the distance between it and the mirror was less than the focal length of the lens, we found that the experiments bore out, in every detail, the results that must follow from the "inequality of curvature theory."

Returning now to fig. 3, in which it was first shown that a converging beam produced an inversion of the phenomenon, we find it impossible to obtain a distinct dark image of the pattern on a light ground by the employment of one converging lens only. This is partly due to the fact that here we are dealing with diverging pencils of light falling on the screen, so that no true image of the pattern is formed; and partly caused by the blurring effect arising from a beam of sunlight, consisting of a number of slightly diverging pencils. This latter may be, to a certain extent, corrected, either by allowing a very small beam of sunlight to fall on the single converging lens, or by causing the sunlight to be brought first to a focus by one lens, and then with a second lens at several feet distance, forming another convergent pencil of light, in which the convergent mirror is placed.

Guided by all that proceeds, we are led to the undoubted conclusion, that the third of the proposed explanations is the correct one, namely, that the whole action of the magic mirror arises from the thicker portions being flatter than the remaining convex surface, and even being sometimes actually concave.

The next question arises, why is there this difference in the curvature of the different portions of the surface? The experience that one gains from an examination of a large number of Japanese mirrors supplies, in part at any rate, the answer to the question. No thick mirror reflects the pattern on the back, not one of the many beautiful mirrors exhibited at the National Exhibition of Japan in 1877, and which we were so fortunate as to be able to experiment with in a darkened room with a bright luminous point at some twelve feet distance, shows the phenomenon in the slightest degree; some good old mirrors in the museum of the Imperial College of Engineering, and which belonged to the family of the late Emperor, the Shogun, of Japan, fail to reflect any trace of a design, and some old round mirrors without handles, which we have also tried, are, with the exception of one about six inches in radius, and for which the owner asked many pounds, equally unsuccessful. Now this in itself, independently of the erroneous idea regarding stamping, is almost sufficient to negative Mr. Prinsep's idea "that part of the metal was by this stamping rendered in a degree harder than the rest, so that in polishing it was not worn away to the same extent." Again, it is not that the pattern is less clearly executed on the backs of these choice mirrors, since the better

the mirror the finer and bolder is the pattern, but what is especially noticeable is that every one of these mirrors is as a whole far thicker than an ordinary Japanese mirror, and its surface is much *less convex*. This naturally led us to inquire, how are Japanese mirrors made convex? are they cast so, or do they acquire this shape from some subsequent process? In the article "Miroirs" in "Les Industries Anciennes et Modernes de l'Empire Chinois," nothing is said on this point, and the paper communicated by M. Julien on the Chinese Magic Mirror to the French Academy, is equally silent on this subject. Professor Pepper says, "Are the mirrors cast in a double mould one side of which is in *intaglio* and one side in *relievo?*" but has no information by which he can answer this question. We also were quite unable to gain any assistance from foreign or from Japanese books or manuscripts regarding the method by which the convexity observed in almost all Japanese mirrors is produced, and were consequently compelled to make inquiries ourselves among mirror makers. Now although shops where mirrors are sold are common enough in Tokio, workshops where they are made are very difficult to find. A workshop was said to exist at Oji, but after a long search in this suburb of Tokio we found only one old woman and a little mercury amalgam in a small hovel about six feet by four, as the representative of the mirror industry. As women are supposed to know nothing in Japan, it was useless to make inquiries of her: another search made on a subsequent occasion in a different direction only elicited the information that mirrors were not made at that time of the year, as the moulds were frostbitten. Mirror-sellers, mirror-polishers we could find, but nobody in Tokio seemed to cast mirrors. We have since found out that this is really the case, since all the common mirrors come from the ancient capital Kioto, about 400 miles to the south of Tokio, and it is only when some special order is given that mirrors are made in the capital. However, at last we lighted on some mirror makers and sellers combined, from whom Mr. Kawaguchi (one of the assistants to the Professor of Natural Philosophy at our College), in the course of many conversations, extracted much valuable information. As a large portion of this is not to be found, as far as we aware, in any books, and as it bears upon the explanation of the magic mirror given in this paper, it naturally finds a place here.

Composition used in Making Mirrors.—In regard to the composition of the mirrors the following seems to be the metal-mixture employed in Tokio:—

Mirrors of First Quality.

Copper	75·2	parts.
Tin	22·6	„
Iyo shirome	2·2	„

Mirrors of Second Quality.

Copper	81·3	parts.
Tin	16·3	,,
Iyo shirome	2·4	,,

Mirrors of Third Quality.

Copper	87·0	parts.
Tin	8·7	,,
Iyo shirome	4·3	,,

Mirrors of Fourth Quality.

Copper	81·3	parts.
Tori shirome	16·3	,,
Iyo shirome	2·4	,,

Mirrors of Fifth Quality.

Copper	71·5	parts.
Tori shirome	28·5	,,

Iyo shirome is the name given to a natural sulphide of lead and antimony taken out of the impurities of the lead ore from the mines of the province Iyo, in the island Shikoku. *Tori shirome* is a shirome containing an admixture of copper. In vol. iv of the "Transactions of the Asiatic Society of Japan," Dr. Geerts gives the metal-mixture employed in one of the largest mirror foundries in Kioto as follows:—

Mirrors of First Quality.

Lead	5	parts.
Tin	15	,,
Copper	80	,,

Mirrors of Inferior Quality.

Lead	10	parts.
Shirome	10	,,
Copper	80	,,

MM. Champion and Pellet give as the result of their analysis of the material of Chinese mirrors:—

Copper	50·8	parts.
Tin	16·5	,,
Zinc	30·5	,,
Lead	2·2	,,

One of the chief of the Tokio mirror makers tells us he never puts ordinary lead into the mixture, since he finds this makes the face of

the mirror very difficult to be amalgamated; also that, in casting, the lead comes to the surface and spoils the mixture. Zinc he also finds has the same effect. But as a small amount of lead is required to be inserted in the composition to prevent the metal from becoming too brittle, the shirome or sulphide of lead and antimony is employed. The chief sources of this shirome arranged in order of merit are the provinces in the south of Japan, called—

1. Iyo, in the island Skikoku,
2. Shekishu,
3. Choshu,
4. Tosa, in the island Shikoku,

but the *shirome* coming from the last province, Tosa, cannot be used for mirrors, as it contains too much lead.

The mirrors of the first quality are only manufactured on receipt of a special order, and new mirrors of even the second and third qualities are rarely found ready made. The ordinary stock of the shops consists of mirrors of the fourth quality, in which there is no tin. The absence of both tin and the Iyo shirome in the composition of the fifth quality is found to make the mirrors give a pale reflection, from the difficulty of amalgamation, and so the fifth composition is not often used.

The composition for the common mirrors is made at the copper mines and forwarded to the various mirror foundries. Formerly the metal for mirrors was extensively prepared at Kioto, but the trade is dying out now, and is said to have been slowly diminishing for the last hundred and thirty years, at the commencement of which period it had reached its maximum.

Moulds for Mirrors.—The most striking feature of the moulds is that *while practically all Japanese mirrors are convex, the surface of each half of the mould is quite flat.* The material used for making the mould is a mixture of a special kind of clay (found near Tokio and Osaka) with water and straw-ash. Two suitable slabs having been formed from this plastic compound with the aid of wooden frames, a thick layer of half liquid mixture of powdered old crucibles, or of a fine powder called *to-no-ko*, made from a soft kind of whetstone, is spread on them. The design for the back of the mirror is then cut directly on one half of the mould, or a sketch drawn on paper is first stuck on and used as a guide in cutting the design in the clay. Sometimes, but rarely, the design is stamped in the clay with a pattern wood-block cut in relief like the proposed back of the mirror. After the design is complete a rim of the same material as that used in the construction of the mould, and having a thickness equal to that desired for the mirror, is attached to one half of the mould. The two halves are then dried in the smoke of a pine tree fire, pressed and tied together, and laid in the casting

box at an angle of 80° with the horizon, the half of the mould on which the design has been cut being uppermost. Finally, the molten speculum metal is run into a number of moulds at the same time, which, when cold, are broken up and the castings removed.

Mirrors cast in a mould, in which the design has been cut by hand, are called *ichi mai buki*, "mould used once," and are regarded as "artists' proofs," as the design on the back is well defined. To form subsequent moulds the two halves are pressed, when the clay is wet, on an *ichi mai buki* mirror, and the pattern is this way transferred, but the designs on the backs of the mirrors cast in such moulds are not as clear as on an *ichi mai buki* mirror, which therefore sells for a much higher price.

Curving the Surface.—The rough mirror is first scraped approximately smooth with a hand-scraping tool, and as this would remove any small amount of convexity, had such been imparted to it in casting, it is useless to make the mould slightly convex. If, however, a convex or concave mirror of small radius is required, then the surface of the mould is made concave or convex. On the other hand, to produce the small amount of convexity which is possessed by ordinary Japanese mirrors the following method is employed, if the mirror is thin, and it is with thin mirrors we have especially to deal, since it is only in these mirrors that the apparent reflection of the back is observed. The mirror is placed face uppermost flat on a wooden board, and then scraped or rather scratched with a rounded iron rod about half an inch in diameter and a foot long, called a *megebo*, "distorting rod," so that a series of parallel scratches is produced, which causes the face of the mirror to become convex in the direction at right angles to the scratches, but to remain straight parallel to the scratches, in fact it becomes very slightly cylindrical, the axis of the cylinder being parallel to the scratches. This effect is very clearly seen by applying a straight-edge in different ways to the face of an unpolished mirror which has received a single set of scratches only. A series of scratches is next made with the *megebo* in a direction of right angles to the former, a third set intermediate between the two former, and so on, the mirror each time becoming slightly cylindrical, the axis of the cylinder in each case being parallel to the line of scratches, so that eventually the mirror becomes generally convex. Some workmen prefer to make the scratches with the *megebo* in the form of small spirals, others in the form of large spirals, but the general principle of the method employed with their mirrors appears to be always the same,—the face of the mirror is scratched with a blunted piece of iron, and becomes slightly convex, the back, therefore, becoming concave.

After the operation with the "distorting rod" the mirror is very slightly scraped with a hand-scraping tool to remove the scratches

and to cause the face to present a smooth surface for the subsequent polishing.

In the case of thick mirrors the convexity is first made by cutting with a knife, and the "distorting rod" applied afterwards. But in connection with this cutting process of thick mirrors there is one very interesting point. If the maker finds on applying from time to time the face of the mirror to a hard clay concave pattern, and turning it round under a little pressure, that a portion of the surface has not been in contact with the pattern, in other words, that he has cut away this portion too much, then he rubs this spot round and round with the *megebo* until he has restored the required degree of convexity. Here again then scratching on the surface produces convexity.

Now, why does the scraping of the "distorting rod" across the face of the mirror leave it convex? During the operation it is visibly concave. The metal must receive then a kind of "buckle," and spring back again so as to become convex when the pressure of the rod is removed. It might in such a case reasonably be expected that the thicker parts of the mirror would yield less to the pressure of the rod than the thinner, and so would be made less convex, or even they might not spring back, on the withdrawal of the rod, and so remain actually concave. Again, since we find that scraping the face of a mirror is the way in which it is made convex, and the back therefore concave, we might conclude that a deep scratch on the back would make the back convex and the face slightly concave. Such a concavity, as we have proved, would explain the phenomenon of the bright line appearing in the reflection of sunlight on the screen which was observed by Professor Atkinson to correspond with the scratch on the back.

It appears then that the magic of the Eastern mirror results from no subtle trick on the part of the maker, from no inlaying of other metals, or hardening of portions by stamping, but merely arises from the natural property possessed by thin bronze of buckling under a bending stress, so as to remain strained in the opposite direction after the stress is removed. And this stress is applied partly by the "distorting rod," and partly by the subsequent polishing, which, in an exactly similar way, tends to make the thinner parts more convex than the thicker.

Polishing.—After the scratches produced by the *megebo* are removed the mirror is first polished with a whetstone called either *iyodo*, "whetstone from the province of Iyo," or *shiroto*, "white whetstone." Afterwards a whetstone called *tenshimado*, "whetstone from the province Tsushima," or the powder *to-no-ko*, previously described, is used. Thirdly, a piece of charcoal, prepared from the *ho* tree (*Magnolia hypoleuca*) is rubbed over the surface. The face now becomes fairly smooth, but it still generally contains some few cavities; these the maker fills up from a stock of copper balls of various sizes which he

has at hand, and which are obtained from the cinders of a copper-furnace. The cavities when thus filled up are well rubbed so as to escape notice, but they may usually be detected by looking at the mirror obliquely.

It was perhaps the presence of these bits of copper in the mirror which Ou-tseu-hing saw broken up in the 13th century, that misled him into concluding that the phenomenon of the magic mirror was produced by the inlaying of denser copper in a portion of the face exactly corresponding with the design on the back.

When the face of the mirror has been made quite smooth, an amalgam consisting, according to the Tokio makers, of half tin and half mercury, with perhaps a trace of lead, or of

Tin	69·36	per cent.,
Mercury	30	„
Lead	0·64	„

according to the analysis of MM. Champion and Pellet ("Industries de l'Empire Chinois") is rubbed over the surface with a stiff straw brush or with the hand. The mirror is finally wiped clean with a soft kind of paper, *mino-gami*, "paper from the province Mino," which is considered to scratch the surface less than silk. Leather was formerly never employed in polishing, as it would have been considered impious to pollute so holy a thing as a mirror by touching it with the skin of an animal; for under the old feudal system in Japan, workers in skins, saddlers, and others, belonged to the Eta or pariah class.

When mirrors possessed by private people require brightening up, in consequence of the surface tarnishing, the paste produced when razors are sharpened on a hone is usually rubbed over the face of the mirror.

III. "On the Torsional Strain which remains in a Glass Fibre after release from Twisting Stress." By J. Hopkinson, D.Sc., F.R.S. Received October 4, 1878.

It has long been known that if a wire of metal or fibre of glass be for a time twisted, and be then released, it will not at once return to its initial position, but will exhibit a gradually decreasing torsion in the direction of the impressed twist. The subject has undergone a good deal of investigation, especially in Germany. The best method of approximating to an expression of the facts has been given by Boltzmann ("Akad. der Wissensch. Wien," 1874). He rests his theory upon the assumption that a stress acting for a short time will

leave after it has ceased a strain which decreases in amount as time elapses, and that the principle of superposition is applicable to these strains, that is to say, that we may add the after-effects of stresses, whether simultaneous or successive. Boltzmann also finds that, if $\phi(t)\tau$ be the strain at time t resulting from a twist lasting a very short time τ, at time $t=0$, $\phi(t)=\frac{A}{t}$, where A is constant for moderate values of t, but decreases when t is very large or very small. A year ago I made a few experiments on a glass fibre which showed a deviation from Boltzmann's law. A paper on this subject by Kohlrausch ("Pogg. Ann.," 1876) suggested using the results of these experiments to examine how Boltzmann's law must be modified to express them. Professor Kohlrausch's results indicate that in the cases of silver wire and of fibre of caoutchouc Boltzmann's principle of superposition is only approximate, and that in the case of a short duration of twisting $\phi(t)=\frac{A}{t^a}$, where a is less than unity; in case of a long duration of twisting he uses other formulæ, which pretty successfully express his results, owing in part no doubt to the fact that in most cases each determination of the constants applies only to the results of one duration of twisting. In a case like the present it appears best to adopt a simple form involving constants for the *material* only, and then see in what way it fails to express the varying conditions of experiment. In 1865 Sir W. Thomson published ("Proceedings of the Royal Society") the results of some experiments on the viscosity of metals, the method being to determine the rate at which the amplitude of torsional vibrations subsided. One of the results was that if the wire were kept vibrating for some time it exhibited much greater viscosity than when it had long been quiescent. This should guard us from expecting to attain great uniformity in experiments so roughly conducted as those of the present paper.

2. The glass fibre examined was about 20 inches in length. Its diameter, which might vary somewhat from point to point, was not measured. The glass from which it was drawn was composed of silica, soda, and lime; in fact, was glass No. 1 of my paper on "Residual Charge of the Leyden Jar" ("Phil. Trans.," 1877. In all cases the twist given was one complete revolution. The deflection at any time was determined by the position on a scale of the image of a wire before a lamp, formed by reflection from a light concave mirror, as in Sir W. Thomson's galvanometers and quadrant electrometer. The extremities of the fibre were held in clamps of cork; in the first attempts the upper clamp was not disturbed during the experiment, and the upper extremity of the fibre was assumed to be fixed; the mirror also was attached to the lower clamp. This arrangement was unsatisfactory, as one could not be certain that a part of the

observed after-effect was not due to the fibre twisting within the clamps and then sticking. The difficulty was easily avoided by employing two mirrors, each cemented at a single point to the glass fibre itself, one just below the upper clamp, the other just above the lower clamp. The upper mirror merely served by means of a subsidiary lamp and scale to bring back the part of the fibre to which it was attached to its initial position. The motion of the lower clamp was damped by attaching to it a vane dipping into a vessel of oil. The temperature of the room when the experiments were tried ranged from 13° C. to 13·8° C., and for the present purpose may be regarded as constant. The lower or reading scale had forty divisions to the inch, and was distant from the glass fibre and mirror 38¾ inches, excepting in Experiment V, when it was at 37½ inches. Sufficient time elapsed between the experiments to allow all sign of change due to after-effect of torsion to disappear. In all cases the first line of the table gives the time in minutes from release from torsion, the second the deflection of the image from its initial position in scale divisions.

Experiment I.—The twisting lasted 1 minute.

t	1	2	3	4	5	7	10	17	25
Scale divisions ..	22	13	9	7	5½	4	3	2	1

Experiment II.—The twisting lasted 2 minutes.

t	1	2	3	4	5	7	10	20	40
Scale divisions..	38	25	18	15	13	10	8	4½	3½

Experiment III.—Twisted for 5 minutes.

t	1	2	3	4	5	7
Scale divisions	64	51	41½	35½	32	26½
....	10	15	22	58	15	
Scale divisions	21½	17	14	7	2	

Experiment IV.—Twisted for 10 minutes.

t	½	1	2	3	4	7	10
Scale divisions	106	85	66	57	49½	37½	31
t	15	25	45	120	170		
Scale divisions	24½	18	13	7	6		

Experiment V.—Twisted for 20 minutes.

t..................	1	2	3	4	5	7	10
Scale divisions	110	89	75	68	61½	52	44
t..................	15	25	40	60	80	100	
Scale divisions......	35½	26½	21	18	13½	12½	

Experiment VI.—Twisted for 121 minutes.

t	$\frac{1}{2}$	1	2	3	4	5	7
Scale divisions .	191	170	148	136	$126\frac{1}{2}$	$119\frac{1}{2}$	$108\frac{1}{2}$
t	10	15	30	65	90	120	589
Scale divisions .	97	$84\frac{1}{2}$	$63\frac{1}{2}$	$41\frac{1}{2}$	34	28	$3\frac{1}{2}$

It should be mentioned that the operation of putting on the twis nd of releasing each occupied about two seconds, and was per- ›rmed half in the second before the epoch $t = 0$, and half in the econd after or as nearly so as could be managed. The time was ıken by ear from a clock beating seconds very distinctly.

3. The first point to be ascertained from these results is whether or ot the principle of superposition, assumed by Boltzmann, holds for ›rsions of the magnitude here used.

If the fibre be twisted for time T through angle X, then the torsion t time t after release will be $X\,\{\psi(T+t)-\psi(t)\}$ where

$$\psi(t) = \int \phi(t)\, dt.$$

If now $T = t_1 + t_2 + t_3 + \ldots$ we may express the effect of one ng twist in terms of several shorter twists by simply noticing that

$$\{\psi(t)-\psi(t+T)\} = X[\{\psi(t)-\psi(t+t_1)\}+\{\psi(t+t_1)-\psi(t+t_1+t_2)\}$$
$$+\{\psi(t+t_1+t_2)-\psi(t+t_1+t_2+t_3)\}+, \&c.]$$

Apply this to the preceding results, calculating each experment from predecessor. Let x_t be the value of $\psi(T+t)-\psi(t)$, that is, torsion at time t, when free, divided by the impressed twist sured in same unit; we obtain the following five tables of com- ion.

Results for T=2 compared with those from T=1.

t	1	2	3	4	5	7
x_t observed....	0·00195	128	092	077	066	051
calculated...	0·00199	112	082	064	051	040
t	10	20	40			
observed....	041	023	018			
calculated ..	029	016				

ults for T=5 compared with those from T=2 and T=1.

t	1	2	3	4	5	7	10
served....	0·00328	262	212	182	164	136	110
culated...	0·00323	233	181	156	136	108	193
t	15	22	58	151			
erved....	087	072	036	010			
ulated...	066	047					

Results for T=10 compared with those from T=5.

t	$\frac{1}{2}$	1	2	3	4	7	10
x_t observed....	0·00544	435	338	292	253	192	159
x_t calculated...	..	469	398	339	300	236	197

t	15	25	45	120	170
x_t observed....	125	092	067	036	031
x_t calculated...	161	130	088		

Results for T=20 compared with those from T=10.

t	1	2	3	4	5	7	10
x_t observed....	0·00580	470	398	358	327	276	234
x_t calculated...	0·00587	483	430	384	356	312	266

t	15	25	40	60	80	100
x_t observed....	188	140	111	085	072	066
x_t calculated...	217	167	135	100	084	

Results for T=121 compared with those from T=20.

t	$\frac{1}{2}$	1	2	3	4	5	7
x_t observed...	0·00979	871	758	697	648	612	556
x_t calculated..	..	1070	950	880	830	780	730

t	10	15	30	65	90	120	589
x_t observed...	497	433	325	212	174	144	18
x_t calculated..	670	600	500	380	350		

In examining these results it must be remembered that those for small values of T are much less accurate than when T is greater, for the quantity observed is smaller but is subject to the same absolute error; any irregularity in putting on or releasing from the stress will cause an error which is a material proportion of the observed deflection. For this reason it would be unsafe to base a conclusion on the experiments with T=1 and T=2. The three last tables agree in indicating a large deviation from the principle of superposition, the actual effect being *less* than the sum of the separate effects of the periods o stress into which the actual period may be broken up. Kohlrausch finds the same to be the case for india-rubber, either greater torsions or longer durations give less after-effects than would be expected from smaller torsions and shorter periods.

4. Assuming with Boltzmann that $\phi(t)=\frac{A}{t}$, we have at time t after termination of a twist lasting time T,

$$x_t=A\{\log(T+t)-\log t\},$$

the logarithms being taken to any base we please. The results were

plotted on paper, x_t being the ordinate and $\log \frac{T+t}{t}$ the abscissa; if the law be true we should find the points all lying on a straight line through the origin. For each value for T they do lie on straight lines very nearly for moderate values of t; but if T is not small these lines pass above the origin. When t becomes large the points drop below the straight line in a curve making towards the origin. This deviation appears to indicate the form $\phi(t)=\frac{A}{t^a}$, a being less than, but near to, unity. If $a=0\cdot95$ we have a fairly satisfactory formula.

$$x_t=A'\left(\overline{T+t}^{\frac{1}{10}}-t^{\frac{1}{10}}\right), \text{ where } A'=\frac{A}{t^a} \text{ when } T=121.$$

In the following Table the observed and calculated values of x_t when T=121 are compared, A' being taken as 0·032.

t	½	1	2	3	4	5	7
x_t observed....	0·00979	871	758	697	648	612	556
x_t calculated...	0·00976	870	755	691	643	600	550

t	10	15	30	65	90	120	589
x_t observed....	497	433	325	212	174	144	18
x_t calculated...	493	429	320	218	176	147	42

To show the fact that A' decreases as T increases if a be assumed constant, I add a comparison when T=20, it being then necessary to take $A'=0\cdot037$.

t	1	2	3	4	5	7	10
x_t observed....	0·00580	470	398	358	327	276	234
x_t calculated...	0·00607	485	422	370	337	285	233

t	15	25	40	60	80	100
x_t observed....	188	140	111	085	072	066
x_t calculated...	185	125	089	067	052	041

A better result would in this case be obtained by assuming $a=0\cdot92$, or $=0\cdot93$ in the former case with $A'=0\cdot021$. Probably the best result would be given by taking A constant, and assuming that a increases with T.

Taking the formula $\phi(t)=\frac{A}{t}$ these experiments give values of A ranging from 0·0017 to 0·0022. Boltzmann for a fibre, probably of a quite different composition, gives numbers from which it follows that A=0·0036.

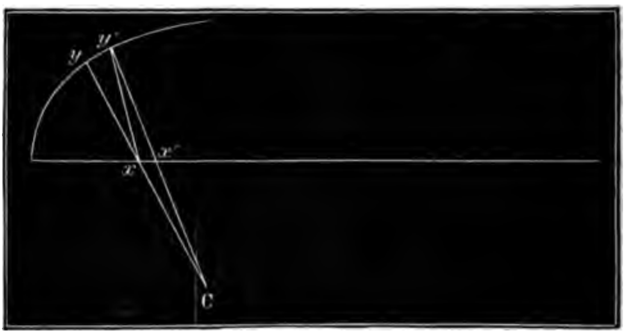
5. In my paper on "Residual Charge of the Leyden Jar" that

subject is discussed in the same manner as Boltzmann discusses the after-effect of torsion on a fibre, and it is worth remarking that the results of my experiments can be roughly expressed by a formula in which $\phi(t)=\frac{A}{t^a}$. For glass No. 5 (soft crown) $a=0{\cdot}65$, whilst for No. 7 (light flint) it is greater; but in the electrical experiment no sign of a definite deviation from the law of superposition was detected.

IV. "Note in correction of an Error in the Rev. Dr. Haughton's Paper 'Notes on Physical Geology. No. V" ("Proc. Roy. Soc.," vol. xxvii, p. 447). By the Rev. Samuel Haughton, M.D., Professor of Geology in the University of Dublin, F.R.S. Received October 9, 1878.

In my paper read 20th June last, and published in the "Journal of the Royal Society," there is an error in p. 450 which I wish to correct.

Referring to the geometrical proof of Mr. Darwin's theorem, I state that from cusp to cusp of the cycloidal wabble occupies $152\frac{1}{2}$ days; this is an error, as it should be 305 days, as can be shown geometrically.



Let yx, $y'x'$, be two successive positions of the line joining the axes of rotation and figure; produce them to meet at C, which will be the centre of curvature, because yx and $y'x'$, are normals to the cycloidal arc yy'; it is well known that yC, (radius of curvature) is double yx (chord of generating circle) or double $y'x$; therefore the angle yxy' is double the angle yCy'; but yxy' measures the angular velocity of the wabble, when x is supposed at rest; therefore the angular velocity of yx is only half that of the wabble, if the axis of figure were at rest. Hence in 305 days, yx will turn through 180° only, and not 360°.

This correction, when introduced into my calculation of Mr. Darwin's problem, p. 182, will double the result, and give 19,350 years to represent the 19,200 years, found by Mr. Darwin.

I would wish to add, that Mr. Darwin, in a letter to myself, proposes to call the cycloidal wabble described by him, a "*lopsided wabble*," as distinguished from the simple circular "*wabble*" described by me; the one being caused by continuous motion of the axis of figure, and the other caused by sudden displacement of that axis.

V. "Measurements of Electrical Constants. No. II. On the Specific Inductive Capacities of Certain Dielectrics." Part I. By J. E. H. GORDON, B.A. Camb. Communicated by Professor J. CLERK MAXWELL, F.R.S. Received October 21, 1878.

(Abstract.)

A paper of mine with the above title was communicated to the Royal Society by Professor J. Clerk Maxwell, F.R.S., on March 9th, 1878. It was read on March 28th, and an abstract of it appeared in the "Proceedings."*

In the course of the summer it was pointed out to me that owing to a mistake in the formula of calculation all the results were wrong. I, therefore, requested permission to withdraw my paper, in order to recalculate the results. The new values of K arrived at led me to make some determinations of refractive indices and to re-write the theoretical deductions at the close of the paper.

I now beg through Professor Maxwell to present the paper in an amended form, in the hope that it may be found not entirely unworthy of the attention of the Royal Society.

As it would be impossible within the limits of an abstract to give any intelligible account of the new method of experiment (due to Professor Maxwell), which has been employed, I will merely give the table of results, reserving all discussion and explanation until the publication of my paper in full.

I may, however, state that the method is a zero method, that the electrified metal plates never touch the dielectrics, and that the electrification, which is produced by an induction coil, has an electromotive force equal to that of about 2,050 chloride of silver cells, and is reversed some 12,000 times per second.

* *Ante*, vol. xxvii, p. 270.

Results.

Dielectric. Glass, Slabs about 1 inch thick.			K.
Chance's optical glass.	Double extra dense flint		3·1639
	Extra dense flint		3·0536
	Light flint		3·0129
	Hard crown		3·1079
Common plate, 2 slabs.	No. 1	3·2581	3·2431
	No. 2	3·2282	
Ebonite, 4 slabs, $\frac{3}{4}, \frac{1}{2}, \frac{1}{2}, \frac{1}{4}$ inch.	No. 1	2·2697	2·2838
	No. 2	2·2482	
	No. 3	2·3097	
	No. 4	2·3077	
Best quality gutta percha			2·4625
Chatterton's compound			2·5474
India-rubber	black		2·2200
	vulcanised		2·4969
Solid paraffin, sp. gr. at 11° C. ·9109. Melting point 68° C. 6 slabs cut in planing machine. Results corrected for cavities.	No. 1	1·9940	Mean...1·9936*
	No. 2	1·9784	
	No. 3	1·9969	
	No. 4	2·0126	
	No. 5	1·9654	
	No. 6	2·0143	
Shellac			2·7464
Sulphur			2·5793
Bisulphide of carbon			1·8096†

The following table compares the refractive indices of the transparent dielectrics with the square roots of the specific inductive capacities. In cases where there is a wide difference μ is taken from books on physics; wherever there was a close agreement it was carefully determined by the author, except in the case of paraffin, where the value is that given in Maxwell's "Electricity."

* Messrs. Gibson and Barclay, "Phil. Trans.," 1871, using a method entirely different from mine, obtained K = 1·977 for paraffin. Correcting for a slight difference of density, I find that if they had used my paraffin their result would have been 1·9833.

† I am not quite certain of the accuracy of this result.

Dielectric.	$\sqrt{K}$.	Nearest value of μ.	Ray for which μ is nearest.
Double extra dense flint glass................	1·7783	1·7460	Band in extreme violet in magnesium spark spectrum.
Extra dense flint.......	1·7474	1·6757	
Light flint............	1·7343	1·5113	
Hard crown...........	1·7629	1·5920	
Plate glass............	1·8009	1·543	
Paraffin...............	1·4119	1·422	Rays of infinite wave length.
Sulphur...............	1·6060	2·115	
Bisulphide of carbon....	1·3456	1·6114	

VI. "Researches in Spectrum Analysis in connexion with the Spectrum of the Sun. No. VII." By J. N. Lockyer, F.R.S. Received December 11, 1878. Read December 12.

Discussion of the Working Hypothesis that the so-called Elements are Compound Bodies.

Part I.

It is known to many Fellows of the Society that I have for the last four years been engaged upon the preparation of a map of the solar spectrum on a large scale, the work including a comparison of the Fraunhofer lines with those visible in the spectrum of the vapour of each of the metallic elements in the electric arc.

To give an idea of the thoroughness of the work, at all events in intention, I may state that the complete spectrum of the sun, on the scale of the working map, will be half a furlong long; that to map the metallic lines and purify the spectra in the manner which has already been described to the Society, more than 100,000 observations have been made and about 2,000 photographs taken.

In some of these photographs we have vapours compared with the sun; in others vapours compared with each other; and others again have been taken to show which lines are long and which are short in the spectra.

I may state by way of reminder that the process of purification consisted in this: When, for instance, an impurity of Mn was searched for in Fe, if the longest line of Mn was absent, the short lines must also be absent on the hypothesis that the elements are elementary; if the longest line were present, then the impurity was traced down to the shortest line present.

Table I. Final reduction—Iron.

Intensity in Sun.	Wave-length and length of line.	Coincidences with Short Lines.
	39	
1	0600 / 2	U / 3, Zr / 5, Yt / 4
3	0622 / 4	Va / 4
2	0920 / 3	Va / 2, Ba / 3
3	1010 / 4	Va / 4, Pt / 3
2	1648 / 2	Co / 3
2	1755 / 3	Mn / 3, Ce / 4
2	1835 / 4	Os / 2
1	2700 / 1	Va / 2
1	2950 / 1	Mo / 3
3	3023 / 4	Ce / 4
5	3435 / 4	U / 2
3	3475 / 2	Ba / 2, Rh / 2
3	3628 / 3	Ta / 3
2	3975 / 2	Co / 3
3	4026 / 3	Va / 5
3	4422 / 4	Mo / 3
3	4720 / 2	Yt / 5, Th / 3
2	5012 / 2	Di / 2
2	5160 / 2	Ce / 3, Ru / 3
2	5210 / 3	W / 3
2	5423 / 4	U / 3, Mo / 3, W / 4
3	6215 / 3	Yt / 5, Ce / 3, Di / 2
2	6571 / 2	Zr / 2
3	6662 / 2	Th / 1
1	7555 / 3	Os / 2, Ta / 4, Cr / 2
3	7578 / 4	Di / 2
2	7685 / 2	Va / 4
2	8083 / 2	Ti / 1
1	8320 / 1	Cr / 3
3	9520 / 3	Ru / 3
2	9750 / 2	Mo / 3

Table II. Final reduction—Titanium.

Intensity in Sun.	Wave-length and length of line.	Coincidences with Short Lines.
1	39 0000 / 3	Zr 4
4	0048 / 3	Th 4
5	1040 / 5	Mn 4, Ce 5, Di 3
2	1360 / 3	Va 4
5	1915 / 8	Ce 4
4	2050 / 3	U 3, La 3
3	2368 / 2	Va 3
3	3718 / 5	Th 4, Ce 4
2	4775 / 1	Fe 2
2	5722 / 1	Zr 1, Rh 3
4	6175 / 2	U 3
3	6335 / 2	Di 3, Ta 5
2	8083 / 1	Fe 2
3	8152 / 2	Mo 3
1	8922 / 1	Mn 4, Cr 4
2	9798 / longest	Va longest

The Hypothesis that the Elements are Simple Bodies does not inclule all the Phenomena.

The final reduction of the photographs of all the metallic elements in the region 39-40—a reduction I began in the early part of the present year, and which has taken six months, summarised all the observations of metallic spectra compared with the Fraunhofer lines accumulated during the whole period of observation. Now this reduction has shown me that the hypothesis that identical lines in different spectra are due to impurities is not sufficient. I shall show in detail in a subsequent paper the hopeless confusion in which I have been landed. I limit myself on the present occasion to giving tables showing how the hypothesis deals with the spectra of iron and titanium.

We find short-line coincidences between many metals the impurities of which have been eliminated, or in which the freedom from mutual impurity has been demonstrated by the absence of the longest lines.

Evidences of Celestial Dissociation.

It is five years since I first pointed out that there are many facts

and many trains of thought suggested by solar and stellar physics which point to another hypothesis, namely, *that the elements themselves, or at all events some of them, are compound bodies.*

In a letter written to M. Dumas, December 3, 1873, and printed in the *Comptes Rendus*, I thus summarised a memoir which has since appeared in the Philosophical Transactions.

"Il semble que plus une étoile est chaude plus son spectre est simple, et que les éléments métalliques se font voir dans l'ordre de leurs poids atomiques.*

"Ainsi nous avons:

"1. Des étoiles très-brillantes où nous ne voyons que l'hydrogène, *en quantité énorme*, et le magnésium;

"2. Des étoiles plus froides, comme notre Soleil, où nous trouvons:

$$H+Mg+Na$$
$$H+Mg+Na+Ca,\ Fe,\ \ldots;$$

dans ces étoiles, pas de métalloïdes;

"3. Des étoiles plus froides encore dans lesquelles *tous les éléments métalliques sont* ASSOCIÉS, où leurs lignes ne sont plus visibles, et où nous n'avons que les spectres des métalloïdes et des composés.

"4. Plus une étoile est âgée, *plus l'hydrogène libre disparaît;* sur la terre, nous ne trouvons plus d'hydrogène en liberté.

"Il me semble que ces faits sont les preuves de plusieurs idées émises par vous. J'ai pensé que nous pouvions imaginer une '*dissociation céleste*,' qui continue le travail de nos fourneaux, et que les métalloïdes sont des composés qui sont dissociés par la température solaire, pendant que les éléments métalliques monatomiques, dont les poids atomiques sont les moindres, son précisément ceux qui résistent, même à la température des étoiles les plus chaudes."

Before I proceed further, I should state that while observations of the sun have since shown that calcium should be introduced between hydrogen and magnesium for that luminary, Dr. Huggins' photographs have demonstrated the same fact for the stars, so that in the present state of our knowledge, independent of all hypotheses, the facts may be represented as follows, the symbols indicating the spectrum of which the lines are visible:—

Hottest Stars	Lines of	H+Ca+Mg	
Sun ..		H+Ca+Mg+Na+Fe	
Cooler Stars		— — Mg+Na+Fe+Bi+Hg	
Coolest ..	Fluted bands of	— — — — — —	Metalloids

* This referred to the old numbers in which Mg=12, Na=23.

Following out these views, I some time since communicated a paper to the Society on the spectrum of calcium, to which I shall refer more expressly in the sequel.

Differentiation of the Phenomena to be observed on the Two Hypotheses.

When the reductions of the observations made on metallic spectra, on the hypothesis that the elements were really elementary, had landed me in the state of utter confusion to which I have already referred, I at once made up my mind to try the other hypothesis, and therefore at once sought for a critical differentiation of the phenomena on the two hypotheses.

Obviously the first thing to be done was to inquire whether one hypothesis would explain these short-line coincidences which remained after the reduction of all the observations on the other. Calling for simplicity's sake the short lines common to many spectra *basic lines*, the new hypothesis, to be of any value, should present us with a state of things in which basic molecules representing bases of the so-called elements should give us their lines, varying in intensity from one condition to another, the *conditions* representing various compoundings.

Suppose A to contain B as an impurity and as an element, what will be the difference in the spectroscopic result?

A in both cases will have a spectrum of its own;

B as an impurity will add its lines according to the amount of impurity, as I have shown in previous papers.

B as an element will add its lines according to the amount of dissociation, as I have also shown.

The difference in the phenomena, therefore, will be that, with gradually-increasing temperature, the spectrum of A *will fade*, if it be a compound body, as it will be increasingly dissociated, and it *will not* fade if it be a simple one.

Again, on the hypothesis that A is a compound body, that is, one compounded of at least two similar or dissimilar molecular groupings, then the longest lines at one temperature will not be the longest at another; the whole fabric of "impurity elimination," based upon the assumed single molecular grouping, falls to pieces, and the origin of the basic lines is at once evident.

This may be rendered clearer by some general considerations of another order.

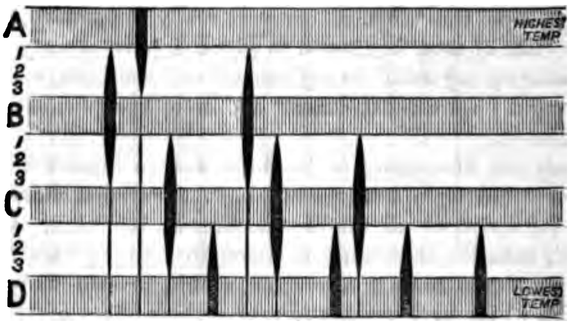
General Considerations.

Let us assume a series of furnaces A . . . D, of which A is the hottest.

Let us further assume that in A there exists a substance a by itself competent to form a compound body β by union with itself or with something else when the temperature is lowered.

Then we may imagine a furnace B in which this compound body exists alone. The spectrum of the compound β would be the only one visible in B, as the spectrum of the assumed elementary body a would be the only one visible in A.

FIG. 1.*



A lower temperature furnace C will provide us with a more compound substance γ, and the same considerations will hold good.

Now if into the furnace A we throw some of this doubly-compounded body γ, we shall get at first an integration of the three spectra to which I have drawn attention; the lines of γ will first be thickest, then those of β; finally a will exist alone, and the spectrum will be reduced to one of the utmost simplicity.

This is not the only conclusion to be drawn from these considerations. Although we have by hypothesis β, γ, and δ all higher, that is, more compound forms of a, and although the strong lines in the diagram may represent the true spectra of these substances in the furnaces B, C, and D, respectively, yet, in consequence of incomplete dissociation, the strong lines of β will be seen in furnace C, and the strong lines of γ will be seen in furnace D, *all as thin lines*. Thus, although in C we have no line which is not represented in D, the intensities of the lines in C and D are entirely changed.

In short, the line of a strong in A is *basic* in B, C, and D, the lines of β strong in B are *basic* in C and D, and so on.

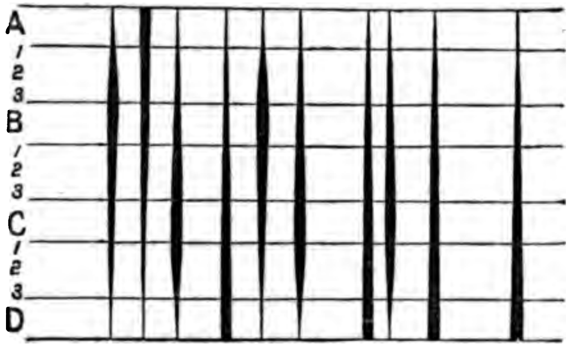
I have prepared another diagram which represents the facts on the supposition that the furnace A, instead of having a temperature sufficient to dissociate β, γ, and δ into a is far below that stage, although higher than B.

It will be seen from this diagram that then the only difference in the spectra of the bodies existing in the four furnaces would consist in the relative thicknesses of the lines. The spectrum of the sub-

* The figures between the hypothetical spectra point to the gradual change as the spectrum is observed near the temperature of each of the furnaces.

stances as they exist in A would contain as many lines as would the spectrum of the substances as they exist in D; each line would in

Fig. 2.



turn be *basic in the whole series of furnaces* instead of in one or two only.

Application of these General Considerations to Impurity Elimination.

Now let us suppose that in the last diagram (Fig. 2) the four furnaces represent the spectra of say, iron, broken up into different finenesses by successive stages of heat. It is first of all abundantly clear that the relative thicknesses of the iron lines observed will vary according as the temperature resembles that of A, B, C, or D. The positions in the spectra will be the same, but the intensities will vary; this is the point. *The longest lines, represented in the diagram by the thickest ones, will vary as we pass from one temperature to another.* It is on this ground that I have before stated that the whole fabric of impurity elimination must fall to pieces on such an hypothesis. Let us suppose, for instance, that manganese is a compound of the form of iron represented in furnace B, with something else; and suppose again that the photograph of iron which I compare with manganese represents the spectrum of the vapour at the temperature of the furnace D. To eliminate the impurity of iron in manganese, as I have eliminated it, we begin the search by looking for the longest and strongest lines shown in the photograph of iron, in the photograph of manganese taken under the same conditions. I do not find these lines. I say, therefore, that there is no impurity of iron in manganese, but although the longest iron lines are not there, some of the fainter basic ones are. This I hold to be the explanation of the apparent confusion in which we are landed on the supposition that the elements are elementary.

Application of these Considerations to Known Compounds.

Now to apply this reasoning to the dissociation of a known compound body into its elements—

A compound body, such as a salt of calcium, has as definite a spectrum as a simple one; but while the spectrum of the metal itself consists of lines, the number and thickness of some of which increase with increased quantity, the spectrum of the compound consists in the main of channelled spaces and bands, which increase in like manner.

In short, the molecules of a simple body and a compound one are affected in the same manner by quantity in so far as their spectra are concerned; *in other words, both spectra have their long and short lines*, the lines in the spectrum of the element being represented by bands or fluted lines in the spectrum of the compound; and in each case the greatest simplicity of the spectrum depends upon the smallest quantity, and the greatest complexity (a continuous spectrum) upon the greatest.

The heat required to act upon such a compound as a salt of calcium so as to render its spectrum visible, dissociates the compound according to its volatility; the number of true metallic lines which thus appear is a measure of the quantity of the metal resulting from the dissociation, and as the metal lines increase in number, the compound bands thin out.

I have shown in previous papers how we have been led to the conclusion that binary compounds have spectra of their own, and how this idea has been established by considerations having for a basis the observations of the long and short lines.

It is absolutely similar observations and similar reasoning which I have to bring forward in discussing the compound nature of the chemical elements themselves.

In a paper communicated to the Royal Society in 1874, referring, among other matters, to the reversal of some lines in the solar spectrum, I remarked :*—

" It is obvious that greater attention will have to be given to the precise *character* as well as to the position of each of the Fraunhofer lines, in the thickness of which I have already observed several anomalies. I may refer more particularly at present to the two H lines 3933 and 3968 belonging to calcium, which are much thicker in all photographs of the solar spectrum [I might have added that they were by far the thickest lines in the solar spectrum] than the largest calcium line of this region (4226·3), this latter being invariably thicker than the H lines in all photographs of the calcium spectrum, and remaining, moreover, visible in the spectrum of substances con-

* "Phil. Trans.," vol. clxiv, part 2, p. 807.

taining calcium in such small quantities as not to show any traces of the H lines.

"How far this and similar variations between photographic records and the solar spectrum are due to causes incident to the photographic record itself, or to variations in the intensities of the various molecular vibrations under solar and terrestrial conditions, are questions which up to the present time I have been unable to discuss.

An Objection Discussed.

I was careful at the very commencement of this paper to point out that the conclusions I have advanced are based upon the analogies furnished by those bodies which, by common consent and beyond cavil and discussion, are compound bodies. Indeed, had I not been careful to urge this point the remark might have been made that the various changes in the spectra to which I shall draw attention are not the results of successive dissociations, but are effects due to putting the same mass into different kinds of vibration or of producing the vibration in different ways. Thus the many high notes, both true and false, which can be produced out of a bell with or without its fundamental one, might have been put forward as analogous with those spectral lines which are produced at different degrees of temperature with or without the line, due to each substance when vibrating visibly with the lowest temperature. To this argument, however, if it were brought forward, the reply would be that it proves too much. If it demonstrates that the *h* hydrogen line in the sun is produced by the same molecular grouping of hydrogen as that which gives us two green lines only when the weakest possible spark is taken in hydrogen inclosed in a large glass globe, it also proves that calcium is identical with its salts. For we can get the spectrum of any of the salts alone without its common base, calcium, as we can get the green lines of hydrogen without the red one.

I submit, therefore, that the argument founded on the overnotes of a sounding body, such as a bell, cannot be urged by any one who believes in the existence of any compound bodies at all, because there is no spectroscopic break between acknowledged compounds and the supposed elementary bodies. The spectroscopic differences between calcium itself at different temperatures is, as I shall show, as great as when we pass from known compounds of calcium to calcium itself. There is a perfect continuity of phenomena from one end of the scale of temperature to the other.

Inquiry into the Probable Arrangement of the Basic Molecules.

As the results obtained from the above considerations seemed to be so far satisfactory, inasmuch as they at once furnished an explanation

of the *basic lines* actually observed, the inquiry seemed worthy of being carried to a further stage.

The next point I considered was to obtain a clear mental view of the manner in which, on the principle of evolution, various bases might now be formed, and then become basic themselves.

It did not seem unnatural that the bases should increase their complexity by a process of continual multiplication, the factor being 1, 2, or even 3, if conditions were available under which the temperature of their environment should decrease, as we imagined it to do from the furnace A down to furnace D. This would bring about a condition of molecular complexity in which the proportion of the molecular weight of a substance so produced in a combination with another substance would go on continually increasing.

Another method of increasing molecular complexity would be represented by the addition of molecules of different origins. Representing the first method by $A + A$, we could represent the second by $A + B$. A variation of the last process would consist in a still further complexity being brought about by the addition of another molecule of B; so that instead of $(A+B)_2$ merely, we should have $A+B_2$.

Of these three processes the first one seemed that which it was possible to attack under the best conditions, because the consideration of impurities was eliminated; the prior work has left no doubt upon the mind about such and such lines being due to calcium, others to iron, and so forth. That is to say, they are visible in the spectra of these substances as a rule. The inquiry took this form: Granting that these lines are special to such and such a substance, does each become basic in turn as the temperature is changed?

I therefore began the search by reviewing the evidence concerning calcium, and seeing if hydrogen, iron, and lithium behaved in the same way.

Application of the above Calcium Views to Calcium, Iron, Lithium, and Hydrogen.

Calcium.

It was in a communication to the Royal Society made in 1874 ("Proc. Roy. Soc.," vol. xxii, p. 380), that I first referred to the possibility that the well-known line spectra of the elementary bodies might not result from the vibration of similar molecules. I was led to make the remark in consequence of the differences to which I have already drawn attention in the spectra of certain elements as observed in the spectrum of the sun and in those obtained with the ordinary instrumental appliances.

Later ("Proc. Roy. Soc.," No. 168, 1876) I produced evidence that the molecular grouping of calcium which, with a small induction coil and small jar, gives a spectrum with its chief line in the blue, is nearly

broken up in the sun, and quite broken up in the discharge from a large coil and jar, into another or others with lines in the violet.

I said "another" or "others," because I was not then able to determine whether the last-named lines proceeded from the same or different molecules; and I added that it was possible we might have to wait for photographs of the spectra of the brighter stars before this point could be determined.

I also remarked that this result enabled us to fix with very considerable accuracy the electric dissociating conditions which are equivalent to that degree of dissociation at present at work in the sun.

In fig. 3 I have collected several spectra copied from photographs, in order that the line of argument may be grasped.

First we see what happens to the non-dissociated and the dissociated chloride. Next we have the lines with a weak voltaic arc, the single line to the right (W. L. 4226·3) is much thicker than the two lines (W. L. 3933 and 3968) to the left, and reverses itself.

We have next calcium exposed to a current of higher tension. It will be seen that here the three lines are almost equally thick, and all reverse themselves.

Now it will be recollected that in the case of known compounds the band structure of the true compounds is reduced as dissociation works its way, and the spectrum of each constituent element makes its appearance. If in 3 we take the wide line as representing the banded spectrum of the compound, and the thinner ones as representing the longest elemental lines making their appearance as the result of partial dissociation, we have, by hypothesis, an element behaving like a compound.

If the hypothesis be true, we ought to be able not only to obtain with lower temperatures a still greater preponderance of the single line, *as we do;* but with higher temperatures a still greater preponderance of the double ones, *as we do.*

I tested this in the following manner: employing photography, because the visibility of the more refrangible lines is small, and because a permanent record of an experiment, free as it must be from all bias, is a very precious thing.

Induced currents of electricity were employed in order that all the photographic results might be comparable.

To represent the lowest temperature, I used a small induction coil and a Leyden jar only just large enough to secure the requisite amount of photographic effect. To represent the highest, I used the largest coil and jar at my disposal. The spark was then taken between two aluminium electrodes, the lower one cup-shaped, and charged with a salt of calcium.

In the figure I give exact copies of the results obtained. It will be seen that with the lowest temperature only the single line (2) and

with the highest temperature only the two more refrangible lines (are recorded on the plate.

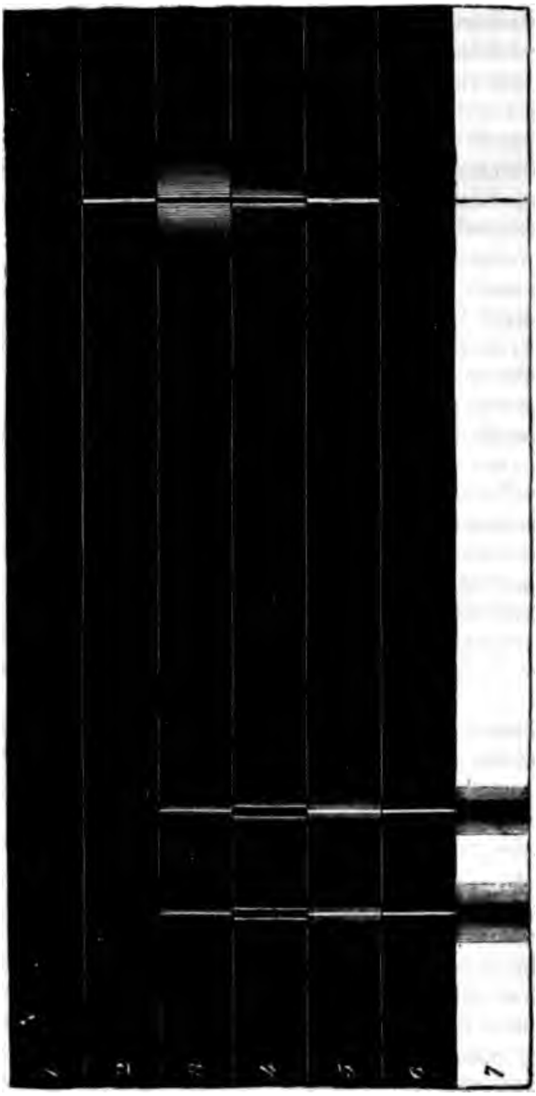


FIG. 3.—The blue end of the Spectrum of calcium under different conditions.

1. Calcium is combined with chlorine ($CaCl_2$). When the temperature is low, the compound molecule vibrates as a whole, the spectrum is at the red end, and no lines of calcium are seen.
2. The line of the metal seen when the compound molecule is dissociated to a slight extent with an induced current.
3. The spectrum of metallic calcium in the electric arc with a small number of cells.
4. The same when the number of cells is increased.
5. The spectrum when a coil and small jar are employed.
6. The spectrum when a large coil and large jar are used.
7.

This proved that the intensity of the vibrations was quite changed in the two experiments.

Perhaps it may not be superfluous here to state the reasons which induced me to search for further evidence in the stars.

It is abundantly clear that if the so-called elements, or more properly speaking their finest atoms—those that give us line spectra—are really compounds, the compounds must have been formed at a very high temperature. It is easy to imagine that there may be no superior limit to temperature, and therefore no superior limit beyond which such combinations are possible, because the atoms which have the power of combining together at these transcendental stages of heat do not exist as such, or rather they exist combined with other atoms, like or unlike, at all lower temperatures. Hence association will be a combination of more complex molecules as temperature is reduced, and of dissociation, therefore, with increased temperature there may be no end.

That is the first point.

The second is this:—

We are justified in supposing that our "calcium," once formed, is a distinct entity, whether it be an element or not, and therefore, by working at it alone, we should never know whether the temperature produces a single simpler form or more atomic condition of the same thing, or whether we actually break it up into X + Y, because neither X nor Y will ever vary.

But if calcium be a product of a condition of relatively lower temperature, then in the stars hot enough to enable its constituents to exist uncompounded, we may expect these constituents to vary in quantity; there may be more of X in one star and more of Y in another; and if this be so, then the H and K lines will vary in thickness, and the extremest limit of variation will be that we shall only have H representing, say, X in one star, and only have K representing, say, Y in another. Intermediately between these extreme conditions we may have cases in which, though both H and K are visible, H is thicker in some and K is thicker in others.

Professor Stokes was good enough to add largely to the value of my paper as it appeared in the "Proceedings" by appending a note pointing out that "When a solid body such as a platinum wire, traversed by a voltaic current, is heated to incandescence, we know that as the temperature increases not only does the radiation of each particular refrangibility absolutely increase, but the proportion of the radiations of the different refrangibilities is changed, the proportion of the higher to the lower increasing with the temperature. It would be in accordance with analogy to suppose that as a rule the same would take place in an incandescent surface, though in this case the spectrum would be discontinuous instead of continuous. Thus, if A, B, C, D, E denote conspicuous bright lines of increasing refrangibility, in the spectrum of the vapour, it might very well be that at a comparatively low temperature A should be the brightest and the most persistent: at a higher temperature, while all were brighter than before, the relative

brightness might be changed, and C might be the brightest and the most persistent, and at a still higher temperature E."

On these grounds Professor Stokes, while he regarded the facts I mentioned as evidence of the high temperature of the sun, did not look upon them as *conclusive* evidence of the dissociation of the molecule of calcium.

Since that paper was sent in, however, the appeal to the stars to which I referred in it has been made, and made with the most admirable results, by Dr. Huggins.

The result of that appeal is, that the line which, according to Professor Stokes's view, should have prevailed over all others, as Sirius is acknowledged to be a hotter star than our sun, if it exists at all in the spectrum, is so faint that it was not recognised by Dr. Huggins in the first instance.

In Sirius, indeed, the H line due to one molecular grouping of calcium is as thick as are the hydrogen lines as mapped by Secchi, while the K line, due to another molecular grouping, which is equally thick in the spectrum of the sun, has not yet made its appearance.

In the sun, where it is as thick as H, the hydrogen lines have vastly thinned.

While this paper has been in preparation, Dr. Huggins has been good enough to communicate to me the results of his most important observations, and I have also had an opportunity of inspecting several of the photographs which he has recently taken. The result of the recent work has been to show that H and *h* are of about the same breadth in Sirius. In *a* Aquilæ, while the relation of H to *h* is not greatly changed, a distinct approach to the solar condition is observed, K being now unmistakably present, although its breadth is small as compared with that of H. I must express my obligations to Dr. Huggins for granting me permission to enrich my paper by reference to these unpublished observations. His letter, which I have permission to quote, is as follows:—

"It may be gratifying to you to learn that in a photograph I have recently taken of the spectrum of *a* Aquilæ there is a line corresponding to the more refrangible of the solar H lines [that is K], but about half the breadth of the line corresponding to the first H lines.

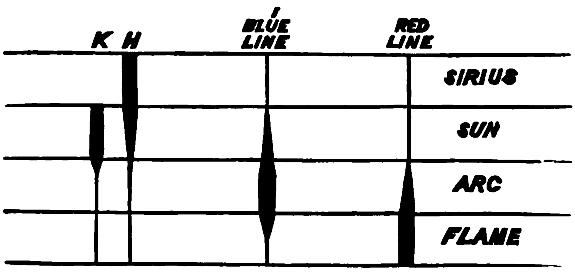
"In the spectra of *a* Lyræ and Sirius the second line is absent."

Professor Young's observations of the chromospheric lines, to which I shall afterwards refer, give important evidence regarding the presence of calcium in the chromosphere of the sun. He finds that the H and K lines of calcium are strongly reversed in every important spot, and that, in solar storms, H has been observed injected into the chromosphere seventy-five times, and K fifty times, while the blue line at W. L. 4226·3, the all-important line at the arc temperature, was only injected thrice.

Further, in the eclipse observed in Siam in 1875, the H and K lines left the strongest record in the spectrum of the chromosphere, while the line near G in a photographic region of much greater intensity was not recorded at all. In the American eclipse of the present year the H and K lines of calcium were distinctly visible at the base of the corona, in which, for the first time, the observers could scarcely trace the existence of any hydrogen.

To sum up, then, the facts regarding calcium, we have first of all the H line differentiated from the others by its almost solitary existence in Sirius. We have the K line differentiated from the rest by its birth, so to speak, in α Aquilæ, and the thickness of its line in the sun, as compared to that in the arc. We have the blue line differentiated from H and K by its thinness in the solar spectrum while they are thick, and by its thickness in the arc while they are thin. We have it again differentiated from them by its absence in solar storms in

Fig. 4.



which they are almost universally seen, and, finally, by its absence during eclipses, while the H and K lines have been the brightest seen or photographed. Last stage of all, we have calcium, distinguished from its salts by the fact that the blue line is only visible when a high temperature is employed, each salt having a definite spectrum of its own, in which none of the lines to which I have drawn attention appear, so long as the temperature is kept below a certain point.

Iron.

With regard to the iron spectrum, I shall limit my remarks to that portion of it visible on my photographic plates, between H and G. It may be described as a very complicated spectrum, so far as the number of lines is concerned, in comparison with such bodies as sodium and potassium, lead, thallium, and the like; but unlike them, again, it contains no one line which is clearly and unmistakably reversed on all occasions. Compared, however, with the spectrum of such bodies as cerium and uranium, the spectrum is simplicity itself.

Now, among these lines are two triplets, two sets of three lines each, giving us beautiful examples of those repetitions of structure in

the spectrum which we meet with in the spectra of almost all bodies, some of which have already been pointed out by Mascart, Cornu, and myself. Now the facts indicate that these two triplets are not due to the vibration of the same molecular grouping which gives rise to most of the other lines. They are as follows. In many photographs in which iron has been compared with other bodies, and in others again in which iron has been photographed as existing in different degrees of impurity in other bodies, these triplets have been seen almost alone, and the relative intensity of them, as compared with the few remaining lines, is greatly changed. In this these photographs resemble one I took three years ago, in which a large coil and jar were employed instead of the arc, which necessitated an exposure of an hour instead of two minutes. In this the triplet near G is very marked; the two adjacent lines more refrangible near it, which are seen nearly as strong as the triplet itself in some of the arc photographs I possess, are only very faintly visible, while dimmer still are seen the lines of the triplet between H and *h*.

There is another series of facts in another line of work. In solar storms, as is well known, the iron lines sometimes make their appearance in the chromosphere. Now, if we were dealing here with one molecular grouping, we should expect the lines to make their appearance in the order of their lengths, and we should expect the shortest lines to occur less frequently than the longest ones. Now, precisely the opposite is the fact. One of the most valuable contributions to solar physics that we possess is the memoir in which Professor C. A. Young records his observation of the chromospheric lines, made on behalf of the United States Government, at Sherman, in the Rocky Mountains. The glorious climate and pure air of this region, to which I can personally testify, enabled him to record phenomena which it is hopeless to expect to see under less favourable conditions. Among these were injections of iron vapour into the chromosphere, the record taking the form of the number of times any one line was seen during the whole period of observation.

Now, two very faint and short lines close to the triplet near G were observed to be injected thirty times, while one of the lines of the triplet was only injected twice.

The question next arises, are the triplets produced by one molecular grouping or by two? This question I also think the facts help us to answer. I will first state, by way of reminder, that in the spark photograph the more refrangible triplet is barely visible, while the one near G is very strong. Now, if one molecular grouping alone were in question, this relative intensity would always be preserved, however much the absolute intensity of the compound system might vary, but if it is a question of two molecules, we might expect that, in some of the regions open to our observation, we should get evidence of cases in

which the relative intensity is reversed or the two intensities are assimilated. What might happen does happen; the relative intensity of the two triplets in the spark photograph is grandly reversed in the spectrum of the sun. The lines barely visible in the spark photograph are among the most prominent in the solar spectrum, while the triplet which is strong in that photograph is represented by Fraunhofer lines not half so thick. Indeed, while the hypothesis that the iron lines in the region I have indicated are produced by the vibration of one molecule does not include all the facts, the hypothesis that the vibrations are produced by at least three distinct molecules includes all the phenomena in a most satisfactory manner.

Lithium.

Before the maps of the long and short lines of some of the chemical elements compared with the solar spectra, which were published in the "Philosophical Transactions" for 1873, Plate 9, were communicated to the Society, I very carefully tested the work of prior observers on the non-coincidence of the red and orange lines of that metal with the Fraunhofer lines, and found that neither of them were strongly, if at all, represented in the sun, and this remark also applies to a line in the blue at wave-length 4603.

The photographic lithium line, however, in the violet, has a strong representative among the Fraunhofer lines.

Applying, therefore, the previous method of stating the facts, the presence of this line in the sun differentiates it from all the others. For the differentiation of the red and yellow lines I need only refer to Bunsen's spectral analytical researches, which were translated in the "Philosophical Magazine," December, 1875.

In Plate 4 two spectra of the chloride of lithium are given, one of them showing the red line strong and the yellow one feeble, the other showing merely a trace of the red line, while the intensity of the yellow one is much increased, and a line in the blue is indicated. Another notice of the blue line of lithium occurs in a discourse by Professor Tyndall, reprinted in the "Chemical News," and in a letter of Dr. Frankland's to Professor Tyndall, dated November 7, 1861. This letter is so important for my argument that I reprint it entire from the "Philosophical Magazine," vol. xxii, p. 472:—

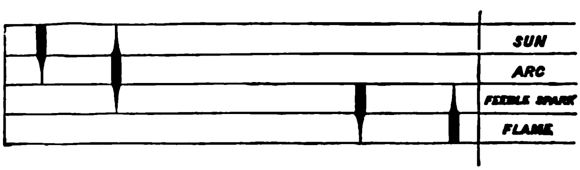
"On throwing the spectrum of lithium on the screen yesterday, I was surprised to see a magnificent blue band. At first I thought the lithic chloride must be adulterated with strontium, but on testing it with Steinheil's apparatus it yielded normal results without any trace of a blue band. I am just now reading the report of your discourse in the 'Chemical News,' and I find that you have noticed the same thing. Whence does this blue line arise? Does it really belong to the lithium, or are the carbon points or ignited air guilty of its pro-

duction? I find these blue bands with common salt, but they have neither the definiteness nor the brilliancy of the lithium band. When lithium wire burns in air it emits a somewhat crimson light; plunge it into oxygen, and the light changes to bluish-white. This seems to indicate that a high temperature is necessary to bring out the blue ray."

Postscript, November 22, 1861.—"I have just made some further experiments on the lithium spectrum, and they conclusively prove that the appearance of the blue line depends entirely on the temperature. The spectrum of lithic chloride, ignited in a Bunsen's burner flame, does not disclose the faintest trace of the blue line; replace the Bunsen's burner by a jet of hydrogen (the temperature of which is higher than that of the Bunsen's burner) and the blue line appears, faint, it is true, but sharp and quite unmistakable. If oxygen now be slowly turned into the jet, the brilliancy of the blue line increases until the temperature of the flame rises high enough to fuse the platinum, and thus put an end to the experiment."

These observations of Professors Tyndall and Frankland differentiate this blue line from those which are observed at low temperatures. The line in the violet to which I have already referred is again differentiated from all the rest by the fact that it is the only line in the spectrum of the sun which is strongly reversed, so far as our present knowledge extends. The various forms of lithium, therefore, may be shown in the following manner.

FIG. 5.



It is remarkable that in the case of this body which at relatively low temperature goes through its changes, its compounds are broken up at the temperature of the Bunsen burner. The spectrum, *e.g.* of the chloride, so far as I know, has never been seen.

Hydrogen.

All the phenomena of variability and inversion in the order of intensity presented to us in the case of calcium can be paralleled by reference to the knowledge already acquired regarding the spectrum of hydrogen.

Dr. Frankland and myself were working together on the subject in 1869. In that year ("Proc. Roy. Soc.," No. 112) we pointed out

that the behaviour of the *h* line was *hors ligne*, and that the whole spectrum could be reduced to one line, F.

"1. The Fraunhofer line on the solar spectrum, named *h* by Ångström, which is due to the absorption of hydrogen, is not visible in the tubes we employ with low battery and Leyden-jar power; it may be looked upon, therefore, as an indication of relatively high temperature. As the line in question has been reversed by one of us in the spectrum of the chromosphere, it follows that the chromosphere, when cool enough to absorb, is still of a relatively high temperature.

"2. Under certain conditions of temperature and pressure, the very complicated spectrum of hydrogen is reduced in our instrument *to one line in the green*, corresponding to F in the solar spectrum."

As in the case of calcium also, solar observation affords us most precious knowledge. The *h* line was missing from the protuberances in 1875, as will be shown from the accompanying extract from the Report of the Eclipse Expedition of that year:—

"During the first part of the eclipse two strong protuberances close together are noticed; on the limb towards the end these are partially covered, while a series of protuberances came out at the other edge. The strongest of these protuberances are repeated three times, an effect of course of the prism, and we shall have to decide if possible the wave-lengths corresponding to the images. We expect *à priori* to find the hydrogen lines represented. We know three photographic hydrogen lines: F, a line near G, and *h*. F is just at the limit of the photographic part of the spectrum, and we find indeed images of protuberances towards the less refrangible part at the limit of photographic effect. For, as we shall show, a continuous spectrum in the lower parts of the corona has been recorded, and the extent of this continuous spectrum gives us an idea of the part of the spectrum in which each protuberance line is placed. We are justified in assuming, therefore, as a preliminary hypothesis, that the least refrangible line in the protuberance shown on the photograph is due to F, and we shall find support of this view in the other lines. In order to determine the position of the next line the dispersive power of the prism was investigated. The prism was placed on a goniometer table in minimum deviation for F, and the angular distance between F and the hydrogen line near G, *i.e.*, $H\gamma$, was found, as a mean of several measurements, to be $3'$. The goniometer was graduated to $15''$, and owing to the small dispersive power, and therefore relatively great breadth of the slit, the measurement can only be regarded as a first approximation. Turning now again to our photographs, and calculating the angular distance between the first and second ring of protuberances, we find that distance to be $3'\ 15''$. We conclude, therefore, that this second ring is due to hydrogen. We, therefore,

naturally looked for the third photographic hydrogen line, which is generally called *h*, but we found no protuberance on our photographs corresponding to that wave-length. Although this line is always weaker than Hγ, its absence on the photograph is rather surprising, if it be not due to the fact that the line is one which only comes out at a high temperature. This is rendered likely by the researches of Frankland and Lockyer ("Proc. Roy. Soc.," vol. xvii, p. 453).

"We now turn to the last and strongest series of protuberances shown on our photographs. The distance between this series and the one we have found reason for identifying with Hγ is very little greater than that between Hβ and Hγ. Assuming the distances equal, we conclude that the squares of the inverse wave-lengths of the three series are in arithmetical progression. This is true as a first approximation. We then calculated the wave-length of this unknown line, and found it to be approximately somewhat smaller than 3,957 tenth-metres. No great reliance can be placed, of course, on the number, but it appears that the line must be close to the end of the visible spectrum.

"In order to decide, if possible, what this line is due to, we endeavoured to find out both by photography and fluorescence whether hydrogen possesses a line in that part of the spectrum. We have not at present come to any definite conclusion. In vacuum tubes prepared by Geissler containing hydrogen, a strong line more refrangible than H is seen, but these same tubes show between Hγ and Hδ, other lines known not to belong to hydrogen, and the origin of the ultra-violet line is therefore difficult to make out. We have taken the spark in hydrogen at atmospheric pressures, as impurities are easier to eliminate, but a continuous spectrum extends over the violet and part of the ultra-violet, and prevents any observation as to lines. We are going on with experiments to settle this point.

"Should it turn out that the line is not due to hydrogen, the question will arise what substance it is due to. It is a remarkable fact that the calculated wave-length comes very close to H. Young has found that these calcium lines are always reversed in the penumbra and immediate neighbourhood of every important sun-spot, and calcium must therefore go up high into the chromosphere. We draw attention to this coincidence, but our photographs do not allow us to draw any certain conclusions.

"At any rate, it seems made out by our photographs that the photographic light of the protuberances is in great part due to an ultra-violet line which does not certainly belong to hydrogen. The protuberances as photographed by this ultra-violet ray seem to go up higher than the hydrogen protuberances, but this may be due to the relative greater length of the line."

In my remarks upon calcium I have already referred to the fact that

the line which our observation led us to believe was due to calcium in 1875, was traced to that element in this year's eclipse. The observations also show the curious connexion that, at the time when the hydrogen lines were most brilliant in the corona, the calcium lines were not detected; next, when the hydrogen lines, being still brilliant, the *h* line was not present (a condition of things which, in all probability, indicated a reduction of temperature), calcium began to make itself unmistakably visible; and finally, when the hydrogen lines are absent, H and K become striking objects in the spectrum of the corona.

To come back to *h*, then, I have shown that Dr. Frankland and myself, in 1869, found that it only made its appearance when a high tension was employed. We have seen that it was absent from among the hydrogen lines during the eclipse of 1875.

I have now to strengthen this evidence by the remark that it is always the shortest line of hydrogen in the chromosphere.

I now pass to another line of evidence.

I submit to the Society a photograph of the spectrum of indium, in which, as already recorded by Thalèn, the strongest line is one of the lines of hydrogen (*h*), the other line of hydrogen (near G) being absent. I have observed the C line in the spark produced by the passage of an induced current between indium poles in dry air.

As I am aware how almost impossible it is to render air perfectly dry, I made the following differential experiment. A glass tube with two platinum poles about half an inch apart was employed. Through this tube a slow current of air was driven after passing through a U-tube one foot high, containing calcic chloride, and then through sulphuric acid in a Wolff's bottle. The spectrum of the spark passing between the platinum electrodes was then observed, a coil with five Grove cells and a medium-sized jar being employed. Careful notes were made of the brilliancy and thickness of the hydrogen lines as compared with those of air. This done, a piece of metallic indium, which was placed loose in the tube, was shaken so that one part of it rested against the base of one of the poles, and one of its ends at a distance of a little less than half an inch from the base of the other pole. The spark was then passed between the indium and the platinum. The red and blue lines of hydrogen were then observed, both by my friend Mr. G. W. Hemming, Q.C., and myself. Their brilliancy was most markedly increased. This unmistakable indication of the presence of hydrogen, or rather of that form of hydrogen which gives us the *h* line alone *associated into* that form which gives us the blue and red lines, showed us that in the photograph we were not dealing with a physical coincidence, but that in the arc this special form of hydrogen had really been present; that it had come from the indium, and that it had registered itself on the photographic plate, although ordinary hydrogen persistently refuses to do so. Although I was

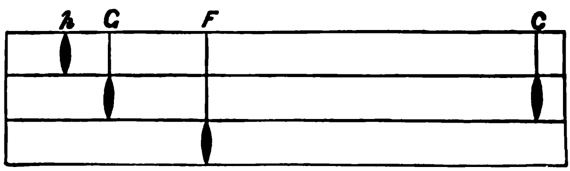
satisfied from former experiments that occluded hydrogen behaves in this respect like ordinary hydrogen, I begged my friend, Mr. W. C. Roberts, F.R.S., Chemist to the Mint, to charge a piece of palladium with hydrogen for me. This he at once did; and I take this present opportunity to express my obligation to him. I exhibit to the Society a photograph of this palladium and of indium side by side. It will be seen that one form of hydrogen in indium has distinctly recorded itself on the plate, while that in palladium has not left a trace. I should add that the palladium was kept in a sealed tube till the moment of making the experiment, and that special precautions were taken to prevent the two pieces between which the arc was taken from becoming unduly heated.

To sum up, then, the facts with regard to hydrogen; we have *h* differentiated from the other lines by its appearance alone in indium; by its absence during the eclipse of 1875, when the other lines were photographed; by its existence as a short line only in the chromosphere of the sun, and by the fact that in the experiments of 1869 a very high temperature was needed to cause it to make its appearance.

With regard to the isolation of the F line I have already referred to other experiments in 1869, in which Dr. Frankland and myself got it alone.* I exhibit to the Society a globe containing hydrogen, which gives us the F line without either the red or the blue one.

The accompanying drawing shows how these lines are integrated in the spectrum of the sun.

Fig. 6.



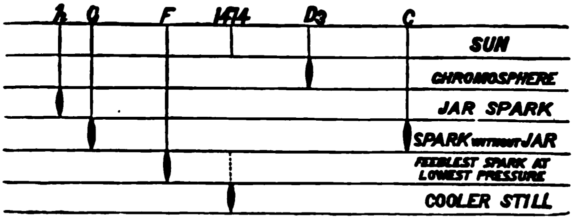
I have other evidence which, if confirmed, leads to the conclusion that the substance which gives us the non-reversed line in the chromosphere and the line at 1474 of Kirchhoff's scale, termed the coronal line, are really other forms of hydrogen. One of these is possibly more simple than that which gives us *h* alone, the other more complex than that which gives us F alone. The evidence on this point is of such extreme importance to solar physics, and throws so much light on star structure generally, that I am now engaged in discussing it, and shall therefore reserve it for a special communication.

In the meantime I content myself by giving a diagram, in which I have arranged the various groupings of hydrogen as they appear to

* See also Plücker, "Phil. Trans.," Part I, p. 21.

exist, from the regions of highest to those of lowest temperature in our central luminary.

FIG. 7.



Summation of the above Series of Facts.

I submit that the facts above recorded are easily grouped together, and a perfect continuity of phenomena established on the hypothesis of successive dissociations analogous to those observed in the cases of undoubted compounds.

The other Branches of the Inquiry.

When we pass to the other possible evolutionary processes to which I have before referred, and which I hope to discuss on a future occasion, the inquiry becomes much more complicated by the extreme difficulty of obtaining pure specimens to work with, although I should remark that in the working hypothesis now under discussion the cause of the constant occurrence of the same substance as an impurity in the same connexion is not far to seek. I take this opportunity of expressing my obligations to many friends who have put themselves to great trouble in obtaining specimens of pure chemicals for me during the whole continuance of my researches. Among these I must mention Dr. Russell, who has given me many specimens prepared by the lamented Matthiessen, as well as some of cobalt and nickel prepared by himself; Professor Roscoe, who has supplied me with vanadium and cæsium alum; Mr. Crookes, who has always responded to my call for thallium; Mr. Roberts, chemist to the Mint, who has supplied me with portions of the gold and silver trial plates and some pieces of palladium; Dr. Hugo Müller, who has furnished me with a large supply of electrolitically-deposited copper; Mr. Holtzman, who has provided me with cerium, lanthanum, and didymium prepared by himself; Mr. George Matthey, of the well-known metallurgical firm of Johnson and Matthey, who has provided me with magnesium and aluminium of marvellous purity; while to Mr. Valentin, Mr. Mellor, of Salford, and other friends, my thanks are due for other substances.

I have already pointed out that a large portion of the work done in

the last four years has consisted in the elimination of the effects of impurities. I am therefore aware of the great necessity for caution in the spectroscopic examination of various substances. There is, however, a number of bodies which permit of the inquiry into their simple or complex nature being made in such a manner that the presence of impurities will be to a certain extent negligable. I have brought this subject before the Royal Society at its present stage in the hope that possibly others may be induced to aid inquiry in a region in which the work of one individual is as a drop in the ocean. If there is anything in what I have said, the spectra of all the elementary substances will require to be re-mapped—and re-mapped from a new standpoint; further, the arc must replace the spark, and photography must replace the eye. A glance at the red end of the spectrum of almost any substance incandescent in the voltaic arc in a spectroscope of large dispersion, and a glance at the maps prepared by such eminent observers as Huggins and Thalén, who have used the coil, will give an idea of the mass of facts which have yet to be recorded and reduced before much further progress can be made.

In conclusion, I would state that only a small part of the work to which I have drawn attention is my own. In some cases I have merely, as it were, codified the work done by other observers in other countries. With reference to that done in my own laboratory I may here repeat what I have said before on other occasions, that it is largely due to the skill, patience, and untiring zeal of those who have assisted me. The burthen of the final reduction, to which I have before referred, has fallen to Mr. Miller, my present assistant; while the mapping of the positions and intensities of the lines was done by Messrs. Friswell, Meldola, Ord and Starling, who have successively filled that post.

I have to thank Corporal Ewings, R.E., for preparing the various diagrams which I have submitted to the notice of this Society.

December 19, 1878.

W. SPOTTISWOODE, M.A., D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Note of an Experiment on the Spectrum of the Electric Discharge." By the Hon. Sir W. R. GROVE, D.C.L., V.P.R.S. Communicated December 19, 1878.

The difference between the appearances at the positive and negative terminal which an electric discharge presents in vacuum tubes has struck many observers. The negative terminal is surrounded in what is called an air vacuum with a blue glow extending to a considerable distance from the platinum wire, and is generally bounded by a dark space separating it from the crimson light of the positive wire; it is affected by the magnet, the light following the direction of the magnetic curves, and a deposit of platinum on the glass tube appears in time in the vicinity of the negative which is absent at the positive terminal. I do not propose to enter more fully on these distinctions which have been largely experimented on by M. Gassiot, Professor Plücker, and to some extent by myself. The recent announcement of Mr. Norman Lockyer of observations on the spectra of bodies which were assumed to be elementary, but which showed lines seeming to denote that they were compound, led me to repeat some old experiments of mine on the spectrum of the electric discharge, one result of which I have ventured to communicate to the Royal Society. I had intended to mention them in the discussion of Mr. Lockyer's paper, but was not able to be present at it.

On November 24th last I examined, with a small spectroscope, by Browning, the electric discharge in some Geissler's air vacuum tubes, three of which I possessed. In these, which were of different shapes and sizes, the effects were the same. The globes into which the negative wire protruded were filled with a blue light more diffused as it became more distant from the wire. The rest of the tube was filled with a crimson light appearing to issue from the positive wire, and this light was striated in the narrow parts of the tubes.

The spectrum from what I will call the positive light presented a series of numerous and variously coloured bands not greatly differing in brightness, and showing what has been called the fluted or channelled spectrum. The spectrum of the negative light was extremely different. Four bright lines divided the spectrum, viz., yellow, green, blue, and violet respectively, the distance between them increasing towards the violet end. There was also a faint line at the extreme red, and the red end of the spectrum was divided into two different tints, terminating with the bright yellow line. In the positive spectrum there was a wide black band, apparently an absorption band, overlapping the yellow and a portion of the orange space.

On looking for a longer time at the spectrum of the negative light, my eye becoming more accustomed to it, I became able to detect other

bands between the bright lines, and on attaching a small prism (with which the spectroscope was provided) in front of the slit, so that the separate spectra of the positive and negative lights could be juxtaposed, I could trace several of the bands which appeared quite distinctly in the positive spectrum, into the negative one; but in the latter they were very faint, while the converse case obtained with the four bright lines I have mentioned, which were brilliant in the negative spectrum and faint or normal in the positive.

Although the four bright lines standing out in strong relief in the negative spectrum was the more striking phenomenon to the eye, yet the black band in the positive appearing in the space corresponding to the bright yellow light in the negative spectrum is equally or possibly more important.

The positive light, far the brightest to the eye, is diffused into a fluted spectrum of substantially equal intensity throughout, while the negative dim light is concentrated into brilliant lines of intense luminosity.

Another tube in which the vacuum was, I have no doubt, produced by the absorption of carbonic acid by potash, and which may be called a carbonic acid vacuum, gave a very different result from the three I have mentioned. In it the light throughout was striated and blue, or bluish, with a slight purple tinge pervading the negative glow. With this tube the spectra were strikingly different from those in the air vacuo. There were in the negative spectrum of this tube six bright lines, viz., extreme red, orange, greenish-yellow, green, greenish-blue, and violet. The same lines with one exception were visible and equally prominent throughout the whole of the tube. That exception, which was noticed by Dr. Frankland, to whom I showed my experiments, was the extreme red line which was apparent only in the spectrum from the negative glow.

On juxtaposing, by means of the prism, this spectrum with the spectrum of the negative light in an air vacuum tube, one only of the lines coincided, viz., the green line, the others were in entirely distinct parts of the spectrum; this was to be expected, as the one tube would give mainly a nitrogen spectrum, the other a carbonic oxide one.

I have long been convinced, and this is now, I believe, the prevalent opinion, that the light of the electric discharge is an incandescence of the intermedium through which it passes, and of the terminals themselves (see "Correlation of Physical Forces," 6th edit., pp. 75, *et seq.*).

If this be so, then, the above experiments, *i.e.*, those on the positive and negative spectra in the same tube, must be either different spectra of the same incandescent substances, or the attenuated gases must be differently decomposed or united in the different parts of the tube, or a different character of electric polarity must ensue in the positive

and negative portions of the gas. The first of the above conditions can only result from difference of heat, which is known to produce different spectra from the same gas. I do not think the effects are due to difference of temperature. It is true that the negative electrode is more heated than the positive in the electric discharge in vacuo, but the heat disseminated by it throughout the negative glow produces in its totality but a slight rise in temperature throughout the volume of the negative glow.

1st. If it be the effect of heat it must be what may be termed molecular heat, as the change in the character of the spectrum being comparatively sudden between the negative and positive light is against the phenomena being caused by change of temperature throughout.

2ndly. Is it caused by chemical decomposition? This is possible, but a different chemical effect pervading two definite portions of the electric discharge is a new effect and not to be hastily assumed. I have shown ("Phil. Trans.," 1852) that the electric discharge has an electro-chemical polarity when acting on attenuated gases, the positive terminal producing an oxidating, and the negative a deoxidating effect; but this effect in my experiments only manifested itself at the terminals, although it may molecularly pervade the gas.

3rdly. Is it due to electric polarity? I incline to think it is, but to a polarity so affecting the molecules of the gas, that, if not actually decomposed, they have something like a chemical polarity impressed upon them. This would to some extent favour Mr. Lockyer's view, though not supporting it to its full extent.

The results may help to explain the phenomena observed in some stars where one or more lines belonging to the spectrum of a given substance is observed, while others are wanting; and if stars have their atmospheres in a state of electric polarity, as is to some extent the case with this earth, or of electric discharge, as is the case with this earth when the Aurora Borealis or Australis is visible, the spectra would differ more or less from those normally observed here. If the spectrum of the negative light were examined through a series of prisms, there can, I think, be little doubt that the very faint intermediate lines would be obliterated by absorption in passing through the glasses, while the bright lines would remain, and thus the spectrum of a nebula would be presented; but it would be but a partial representation of the true spectrum, and the line spectrum seen in the nebulæ may thus be a partial spectrum.

P.S. December 23.

My attention has been called to Mr. De La Rue's paper recently printed in the "Phil. Trans.," which, although he kindly sent me a copy, I had not read when I made the above communication. He finds

in the spectra of hydrogen vacua a notable difference in the lines seen in the negative light, sometimes all and sometimes only one of the recognised lines of hydrogen being visible in that, and in many cases not visible in other parts of the tube. I had tried an experiment with a hydrogen vacuum tube of Geissler; but in that the difference was but slight between the positive and negative lights, though it was very great between the light in the narrow central part of the tube and in the wide portions on each side of it, the crimson light in the narrow tube giving a brilliant three-line spectrum, and the blue light, both on the positive and negative side, giving a comparatively dim fluted spectrum of many bands. The difference between the light of narrow and wide parts of the vacuum tubes has, I believe, been noticed; it is in this case the converse of the effects observed by me in the air vacuum.

II. "On the Precession of a Viscous Spheroid, and on the Remote History of the Earth." By George H. Darwin, M.A., Fellow of Trinity College, Cambridge. Communicated by J. W. L. Glaisher, F.R.S. Received July 22, 1878.

(Abstract.)

This paper is a continuation of a previous one on the bodily tides of homogeneous viscous spheroids (read on May 23rd), and it contains the investigation of the rotation of such a body as modified by the tides raised in it by external disturbing bodies. The earth is taken as the type of the rotating body, and the sun and moon as types of the disturbing ones; this plan not only affords a useful vocabulary, but permits an easy transition from questions of abstract dynamics to those of direct applicability to the physical history of the earth.

In the paper on tides it was shown that, if the disturbing influence be expressed as a potential, which is expanded as a series of solid harmonics, each multiplied by a simple time harmonic, then each such term in the expansion corresponds with a tide in a viscous or imperfectly elastic sphere, which is independent of the tides corresponding to all other terms. Also the height of every such tide is expressible as a fraction of the corresponding equilibrium tide of a perfectly fluid spheroid, and the tide is subject to a retardation which is a function of the frequency of the generating term, and of the constants expressive of the physical constitution of the distorted spheroid.

The case of the moon, supposed to move in a circular orbit in the ecliptic, is treated first. The tide generating potential of that body (of the type $\cos^2-\frac{1}{3}$*) has first to be expanded in the desired form;

* Terms of higher orders are shown to be negligeable.

and then a formula expressive of the shape of the distorted spheroid may be at once written down.

The spheroid or earth is found to be distorted by tides of seven different periods; three nearly semi-diurnal, three nearly diurnal, and one fortnightly tide.

Each such tide has a height which is a different fraction of the corresponding equilibrium tide of a perfectly fluid spheroid, and is differently altered in phase. Throughout nearly the whole investigation it is, however, sufficiently accurate, if the three semi-diurnal tides are grouped together, and so also with the three diurnal tides; by this approximation the earth is regarded as distorted by only three tides.

The next process is the formation of the couples acting on the earth, which are caused by the attraction of the moon on the several tidal protuberances. In the development of these couples only those terms are retained which can give rise to secular alterations in the precession, the obliquity to the ecliptic, and the length of day. These expressions are then substituted in the differential equations of motion, and the equations are integrated; whence follow the correction to the uniform precession of the earth considered as a rigid body, and differential equations expressive of the rate of change of obliquity, and the rate of retardation of the earth's diurnal rotation.

It appears that, if the tides do not lag (as with a perfectly fluid or perfectly elastic spheroid), the obliquity and rotation are unaffected, and, whether they lag or not, the correction to the precession is but a small fraction of the whole precession.

Henceforth it is only the changes of obliquity and rotation which are of interest.

A second disturbing body—the sun—is now introduced. A new set of bodily tides are of course raised, and the expressions for the couples are augmented by the addition of solar terms, and also by terms depending on the attraction of the sun on the lunar tides and the moon on the solar tides. It seems paradoxical that there should be these combined effects, for the sun's and moon's periods have no common multiple. But, as far as concerns their interaction, the sun and moon may be conceived to be replaced by two annular satellites of masses equal to those of the two bodies. The combined effects vanish with the obliquity, and depend solely on those tides which run through their periods in a sidereal day, and in twelve sidereal hours. Up to this point all the analysis is conducted so that the solutions may be applied either to a viscous, elastic, or imperfectly elastic spheroid.

In the case where the earth is purely viscous a graphical examination of the equation, giving the rate of change of obliquity, shows that the obliquity sometimes tends to increase and sometimes to diminish, according as the obliquity and viscosity vary. There are also a number of positions of dynamical equilibrium, some stable and some unstable;

but it would be necessary to give a figure, and to go into details, to give the results satisfactorily.

A similar examination of the equation, giving the retardation of the earth's rotation, shows that there is not so much variety of result, for the tidal friction always tends to retard the earth.

This completes the consideration of the instantaneous effects on the earth, and the next point demanding attention is the reaction, which the bodily tides have upon the disturbing bodies.

The problem is solved by the consideration that however the three bodies may interact the resultant moment of momentum of the moon-earth system remains constant, except in so far as it is affected by the sun's action on the earth. The application of this principle results in an equation giving the rate of increase of the square root of the moon's distance in terms of the heights and retardations of the several bodily tides on the earth; it appears that all the tides, except the fortnightly one, tend to make the moon's distance increase with the time, but the fortnightly tide acts in the opposite direction; its effect is, however, in general very small compared with that of the other tides. It is proved, also, that the tidal reaction on the sun, which goes to modify the earth's orbit, has quite insignificant effects, and may be neglected.

I will now show, from geometrical considerations, how some of the results previously stated come to be true. It will not, however, be possible to obtain a quantitative estimate in this way.

The three following propositions do not properly belong to an abstract, since they are not given in the paper itself; they merely partially replace the analytical method pursued therein. The results of the analysis were so wholly unexpected in their variety, that I have thought it well to show that the more important of them were conformable to common sense. These general explanations might doubtless be multiplied by some ingenuity, but it would not have been easy to discover the results, unless the way had been first shown by analysis.

Prop. I. *If the viscosity be small the earth's obliquity increases, the rotation is retarded, and the moon's distance and periodic time increase.*

The figure represents the earth as seen from above the South Pole, so that S is the Pole, and the outer circle the Equator. The earth's rotation is in the direction of the curved arrow at S. The half of the inner circle which is drawn with a full line is a semi-small-circle of S. lat., and the dotted semi-circle is a semi-small-circle in the same N. lat.

Generally dotted lines indicate parts of the figure which are below the plane of the paper.

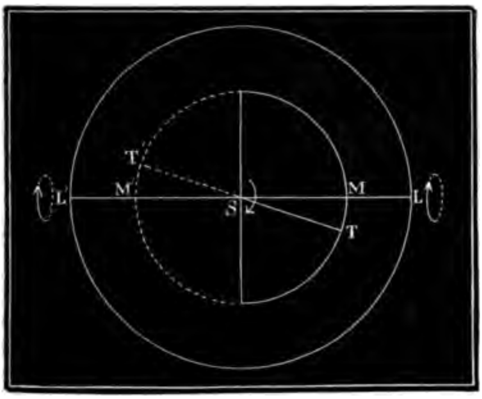
It will make the explanation somewhat simpler, if we suppose the tides to be raised by a moon and anti-moon diametrically opposite to one another. Then let M and M′ be the projections of the moon and anti-moon on to the terrestrial sphere.

If the substance of the earth were a perfect fluid or perfectly elastic, the apices of the tidal spheroid would be at M and M′. If, however, there is internal friction due to any sort of viscosity, the tides will lag, and we may suppose the tidal apices to be at T and T′.

Now, suppose the tidal protuberances to be replaced by two equal heavy particles at T and T′, which are instantaneously rigidly connected with the earth. Then the attraction of the moon on T is greater than on T′; and of the anti-moon on T′ is greater than on T. The resultant of these forces is clearly a pair of forces acting on the earth in the direction of TM, T′M′.

The effect on the obliquity will be considered first.

These forces TM, T′M′, clearly cause a couple about the axis in the equator, which lies in the same meridian as the moon and anti-moon. The direction of the couple is shown by the curved arrows at L, L′.



Now, if the effects of this couple be compounded with the existing rotation of the earth, according to the principle of the gyroscope, it will be seen that the South Pole S tends to approach M, and the North Pole to approach M′. Hence supposing the moon to move in the ecliptic, the inclination of the earth's axis to the ecliptic diminishes, or the obliquity increases.

Next, the forces TM, T′M′, clearly produce a couple about the earth's polar axis, which tends to retard the diurnal rotation.

Lastly, since action and reaction are equal and opposite, and since the moon and anti-moon cause the forces TM, T′M′, on the earth, therefore the earth must cause forces on those two bodies (or on their equivalent single moon) in the directions MT and M′T′. These forces are in the direction of the moon's orbitual motion, and therefore her linear velocity is augmented. Since the centrifugal force of her orbitual motion must remain constant, her distance increases, and with

the increase of distance comes an increase of periodic time round the earth.

This general explanation remains a fair representation of the state of the case so long as the different harmonic constituents of the aggregate tide-wave do not suffer very different amounts of retardations; and this is the case so long as the viscosity is not great.

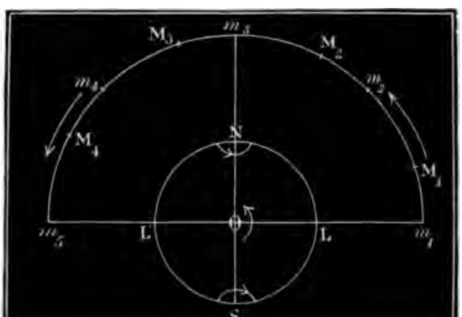
Prop. II. *The attraction of the moon on a lagging fortnightly tide causes the earth's obliquity to diminish, but does not affect the diurnal rotation; the reaction on the moon causes a diminution of her distance, and periodic time.*

The fortnightly tide of a perfectly fluid earth is a periodic increase and diminution of the ellipticity of figure; the increment of ellipticity varies as the square of the sine of the obliquity of the equator to the ecliptic, and as the cosine of twice the moon's longitude from her node. Thus the ellipticity is greatest when the moon is in her nodes, and least when she is 90° removed from them.

In a lagging fortnightly tide the ellipticity is greatest some time after the moon has passed the nodes, and least an equal time after she has passed the point 90° removed from them.

The effects of this alteration of shape may be obtained by substituting for these variations of ellipticity two attractive or repulsive particles, one at the North Pole and the other at the South Pole of the earth. These particles must be supposed to wax and wane, so that when the real ellipticity of figure is greatest they have their maximum repulsive power, and when least they have their maximum attractive power; and their positive and negative maxima are equal.

We will now take the extreme case when the obliquity is 90°; this makes the fortnightly tide as large as possible.



Let the plane of the paper be that of the ecliptic, and let the outer semicircle be the moon's orbit, which she describes in the direction of

the arrows. Let NS be the earth's axis, which lies by hypothesis in the ecliptic, and let LL′ be the nodes of the orbit. Let N be the North Pole; that is to say, if the earth were turned about the line LL′, so that N rises above the plane of the paper, the earth's rotation would be in the same direction as the moon's orbitual motion.

First consider the case where the earth is perfectly fluid, so that the tides do not lag.

Let m_2, m_4 be points in the orbit whose longitudes are 45° and 135°; and suppose that couples acting on the earth about an axis at O perpendicular to the plane of the paper are called positive when they are in the direction of the curved arrow at O. Then, when the moon is at m_1 the particles at N and S have their maximum repulsion. But at this instant the moon is equidistant from both, and there is no couple about O. As, however, the moon passes to m_2 there is a positive couple, which vanishes when the moon is at m_2, because the particles have waned to zero. From m_2 to m_3 the couple is negative; from m_3 to m_4 positive; and from m_4 to m_5 negative. Now, the couple goes through just the same changes of magnitude, as the moon passes from m_1 to m_2, as it does while the moon passes from m_4 to m_5, but in the reverse order; the like may be said of the arcs m_2m_3 and m_3m_4. Hence it follows that the average effect, as the moon passes through half its course, is *nil*, and therefore there can be no secular change in the position of the earth's axis.

But now consider the case when the tide lags. When the moon is at m_1 the couple is zero, because she is equally distant from both particles. The particles have not, however, reached their maximum of repulsiveness; this they do when the moon has reached M_1, and they do not cease to be repulsive until the moon has reached M_2. Hence, during the description of the arc m_1M_2, the couple round O is positive.

Throughout the arc M_2m_3 the couple is negative, but it vanishes when the moon is at m_3, because the moon and the two particles are in a straight line. The particles reach their maximum of attractiveness when the moon is at M_3, and the couple continues to be positive until the moon is at M_4.

Lastly, during the description of the arc M_4m_5 the couple is negative.

But now there is no longer a balance between the arcs m_1M_2 and M_4m_5, nor between M_2m_3 and m_3M_4. The arcs during which the couples are positive are longer and the couples are more intense than in the rest of the semi-orbit. Hence the average effect of the couples is a positive couple, that is to say, in the direction of the curved arrow round O.

It may be remarked that if the arcs m_1M_1, m_2M_2, m_3M_3, m_4M_4 had been 45°, there would have been no negative couples at all, and the positive couples would merely have varied in intensity.

Now, a couple round O in the direction of the arrow, when combined with the earth's rotation, would, according to the principle of the gyroscope, cause the pole N to rise above the plane of the paper, that is to say, the obliquity of the ecliptic would diminish. The same thing would happen, but to a less extent, if the obliquity had been less than 90°; it would not, however, be nearly so easy to show this from general considerations.

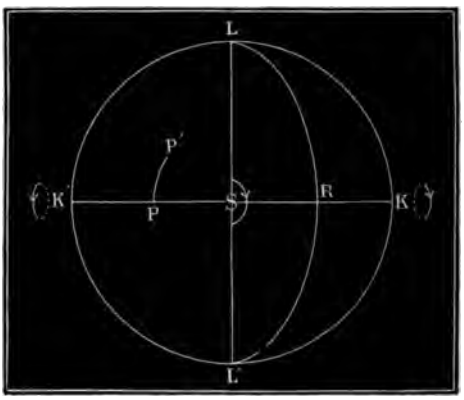
Since the forces which act on the earth always pass through N and S, therefore there can be no moment about the axis NS, and the rotation about that axis remains unaffected. This can hardly be said to amount to strict proof that the diurnal rotation is unaffected by the fortnightly tide, because it has not been rigorously shown that the two particles at N and S are a complete equivalent to the varying ellipticity of figure.

Lastly, the reaction on the moon must obviously be in the opposite direction to that of the curved arrow at O; therefore there is a force retarding her linear motion, the effect of which is a diminution of her distance and of her periodic time.

The fortnightly tidal effect must be far more efficient for very great viscosities than for small ones, for, unless the viscosity is very great, the substance of the spheroid has time to behave sensibly like a perfect fluid, and the tide hardly lags at all.

Prop. III. *An annular satellite not parallel to the planet's equator attracts the lagging tides raised by it, so as to diminish the inclination of the planet's equator to the plane of the ring, and to diminish the planet's rotation. The effects of the joint action of sun and moon may be explained from this.*

Suppose the figure to represent the planet as seen from vertically over the South Pole S; let LL′ be the nodes of the ring, and LRL′ the projection of half the ring on to the planetary sphere.



If the planet were perfectly fluid the attraction of the ring would

luce a ridge of elevation all along the neighbourhood of the arc L', together with a compression in the direction of an axis perpen-lar to the plane of the ring. This tidal spheroid may be conceived e replaced by a repulsive particle placed at P, the pole of the ring, an equal repulsive particle at its antipodes, which is not shown in figure.

ow suppose that the spheroid is viscous, and that the tide lags; since the planet rotates in the direction of the curved arrow at S, repulsive particle is carried past its place, P, to P'. The angle PSP' measure of the lagging of the tide.

'e now have to consider the effect of the repulsion of the ring on a icle which is instantaneously and rigidly connected with the planet '.

nce P' is nearer to the half L of the ring, than to the half L', the ral effect of the repulsion must be a force somewhere in the direc- P'P.

ow this force P'P must cause a couple in the direction of the ed arrows K, K' about an axis, KK', perpendicular to LL', the s of the ring. The effects of this couple, when compounded with planet's rotation, is to cause the pole S to recede from the ring '. Hence the inclination of the planet's equator to the ring nishes.

condly, the force P'P produces a couple about S, adverse to the t's rotation about its axis S. If the obliquity of the ring be this couple will be small, because P' will lie close to S.

tly, it may be shown analytically that the tangential force on g in the direction of the planet's rotation, corresponding with al friction, is exactly counterbalanced by a tangential force in osite direction, corresponding with the change of the obliquity. he diameter of the ring remains constant. It would not be y to prove this from general considerations.

y be shown that, as far as concerns their joint action, the sun n may be conceived to be replaced by a pair of rings, and gs may be replaced by a single one; hence the above propo- also applicable to the explanation of the joint action of the s on the earth, and numerical calculation shows that these ts exercise a very important influence on the rate of variation y.

1 to the paper: the retardation of the earth's rotation would parent acceleration of the moon, if that body were unaffected, artly counterbalanced by the true retardation of the moon. 'e the means of connecting an apparent acceleration of the e heights and retardations of the several bodily tides. I this idea to the supposition that the moon is subject to an

apparent acceleration of 4″ per century, and I found that, if the earth were purely viscous, the moon must be undergoing a secular retardation of 3·6″ per century, while the earth (considered as a clock) must be losing fourteen seconds in the same time. The obliquity also must be diminishing at the rate of 1° in 470 million years.

Under these circumstances the earth must have so great an effective rigidity that the bodily semi-diurnal and diurnal tides would be quite insensible; the bodily fortnightly tide would however be so considerable that the oceanic fortnightly tide would be reduced to one-seventh of its theoretical value on a rigid nucleus, and the time of high water would be accelerated by three days.

The supposition that the earth is a nearly perfectly elastic spheroid leads to very different results in this respect, which, however, I will now pass over.

From this and other considerations, I conclude that a secular acceleration of the moon's motion affords no datum for determining the present amount of tidal friction.

Sir W. Thomson has discussed the probable age of the earth from considering the tidal friction, and he derived his estimate of the rate at which the earth's diurnal rotation is slackening, principally from the secular acceleration of the moon. He fully admitted that his data did not admit of precise results, but, if I am correct, it certainly appears that his argument loses some of its force.

The differential equations, which have to be solved in order to investigate the secular changes in the configuration of the three bodies, are exceedingly complex, and I was only able to solve them by a laborious method, depending partly on analysis and partly on numerical quadratures.

The solution was only applicable to the case where the earth is a purely viscous body, and the numerical value chosen for the coefficient of viscosity was such that the changes proceed with about the maximum rapidity. Starting with the present values of the obliquity, day, month, and year, the changes were traced backwards in time. As we go backwards we find the year sensibly constant, but the obliquity, day and month all diminishing—the last with far the greatest rapidity. The changes proceed at a rapidly increasing rate, as in the retrospect the moon approaches the earth.

At the point where I found it convenient to stop in the first method of solution, about 56 million years have been traversed backwards, and the obliquity is found to have diminished by 9°, the day is found to have fallen to 6 hrs. 50 mins., and the sidereal month to only 1 day 14 hrs.

It is a question of great interest to geologists to determine whether any part of changes of this kind can have taken place during geological history; and I conclude that it might be so. The physical meaning

of the coefficient of viscosity which is used in this solution is as follows:—If a slab of the materials of the earth an inch thick have one face held fixed, and if the other face be subjected to a tangential stress of 13½ tons to the square inch for 24 hours, then the two faces have been displaced relatively to one another through one-tenth of an inch. Such a material would in ordinary parlance be called a solid, and in the tidal problem this must be regarded as a moderately small viscosity, whence I conclude that the earth may have been habitable, and yet have undergone these changes.

Amongst the conclusions of interest to geologists is the following: namely, that the amount of heat generated in the interior of the earth by internal friction, during these 56 million years, would be sufficient, if applied all at once, to heat the whole earth's mass 1,755° F., supposing the earth to have the specific heat of iron. If then it is permissible to suppose that any considerable part of these changes has taken place during geological history, the estimate of the age of the earth, which is founded on the assumption that the earth is simply a cooling sphere, would have to undergo modification.

A second solution of the differential equations is next given, adapted to the hypothesis that the earth stiffened as it cooled; but no definite law of stiffening is assumed. This solution follows a line closely similar to that of the last up to the point where the day has fallen to 6 hrs. 50 mins. The obliquity is, however, found to decrease slightly more than in the previous solution.

At this point it was found necessary to abandon the approximation by which the three semi-diurnal and the three diurnal tides are classified together. The problem then becomes much more complex, and a new method of solution is required.

It is found that in the retrospect the obliquity will only continue to diminish a little beyond the point already reached; for when the month has become equal to twice the day there is no longer a tendency to diminution, and for smaller values of the month the tendency is reversed. This shows that for values of the month less than twice the day, the position of zero obliquity of the earth's axis is dynamically stable. The whole diminution of obliquity, from the initial state back to the critical point of relationship between the month and day, is found to be 10°.

After considering the various discrepancies between the ideal problem solved and the real case of the earth, I conclude that while a large part of the obliquity may be probably referred to these causes, yet that there probably remains an outstanding part which is not so explicable.

The obliquity to the ecliptic is now set on one side, and from a consideration of the equation of conservation of moment of momentum, the initial state is determined, towards which the solution has been

running back. It is found that the initial condition is one in which moon and earth rotate, as though fixed together, in 5 hrs. 40 mins.; and that this condition is one of dynamical instability, so that the moon must either have fallen into the earth, or have receded from it, and have then gone through the changes which were traced backwards.

From this and other considerations it is concluded that, if the moon and earth were ever molten viscous masses, then it is highly probable that they once formed parts of a common mass.

The rest of the paper is occupied with a number of miscellaneous propositions, and with a discussion of the physical significance of the results obtained.

I will here only mention that the case of the Martian satellites appears to me a very striking corroboration of the applicability of these views to the solar system, whilst the Uranian system of satellites is, at first sight, unfavourable.

A whole series of problems, some of them of great difficulty, still await solution; and not until they are solved will it be possible either decisively to accept or reject the modified form of the nebular hypothesis, to which my results obviously point.

(Postscript.) Added November 8th, 1878.

A subsequent investigation has shown that, although the amount of heat which might be generated by internal friction in the earth might be very great, yet its distribution would be such that it could scarcely sensibly affect Sir W. Thomson's investigation of the secular cooling of the earth.

III. "Problems connected with the Tides of a Viscous Spheroid." By G. H. Darwin, M.A., Trinity College, Cambridge. Communicated by J. W. L. Glaisher, F.R.S. Received November 14, 1878.

(Abstract.)

In this paper certain problems are treated, which were alluded to in two previous papers on the Tides and Precession of a viscous spheroid.* For brevity the spheroid is spoken of as the earth, and the disturbing body as the moon.

I. *Secular Distortion of the Spheroid, and certain Tides of the second order.*

The distortion arises from the unequal distribution of the tidal frictional couple over the surface of the spheroid.

* Read before the Royal Society on May 23 and December 19 respectively.

In forming the theory of tides, it was assumed that the action of the tidal protuberance on any element of the surface of the mean sphere was entirely normal to the sphere, and consisted of the weight of the prismatic element of the tidal protuberance, which stands on the element of surface. This is not rigorously correct, because, if it were so, there would be no couples tending to alter the diurnal rotation and obliquity of the earth. The effects of these couples were considered in the paper on "Precession," but the tidal protuberance was there assumed to be instantaneously rigidly connected with the mean sphere. The present problem is concerned with the non-rigid attachment of the protuberance to the sphere.

A sphere is supposed to be distorted into any form differing infinitesimally from the true sphere, and to be acted on by any external disturbing potential. It is then found what tangential stress must be supposed to act across the base of any prismatic element of the protuberance, in order that the equilibrium of that element may be maintained, the pressures transmitted by the four contiguous elements being taken into account. It appears that if the protuberance has the equilibrium form, due to the external disturbing potential, then there is no tangential stress between the true sphere and the protuberance. But since the tides of a viscous spheroid lag, the form of the viscous tidal protuberance is not one of equilibrium, and there is such a tangential stress across the base of each element of the protuberance. It is obvious that these tangential stresses may produce a continued distortion of the spheroid.

The problem, as applicable to the earth, is treated in the simple case where the obliquity to the ecliptic is zero, and where there is only one disturbing body or moon.

The sum of the moments of the tangential stresses about the axis of rotation gives the tidal frictional couple, and its form is found to agree with that found by a different method in the paper on "Precession."

When the earth's rotation is taken into account, it appears that the component along the meridian of tangential stress at any point of the surface is periodic in time; whilst one part of the component perpendicular to the meridian is periodic, and the other non-periodic. The periodic parts of the component tangential stresses give rise to small tides of the second order (varying as the square of the tide-generating force), and are neglected, but the non-periodic part gives rise to a secular distortion.

Since the earth's rotation as a whole is retarded, therefore the distorting tangential stresses all over the surface constitute a non-equilibrating system of forces, and in order to find the distortion of the globe, they must be deemed to be equilibrated by the effective forces due to the inertia of the slackening diurnal rotation. These

effective forces give bodily forces in the interior, the sum of whose moments about the axis of rotation is equal and opposite to the tidal frictional couple. The problem is thus reduced to finding the distortion of a sphere subject to bodily force equilibrated by surface action, and it is solved by Sir W. Thomson's method of finding the internal strain of an elastic sphere under like conditions, although here the bodily force has no corresponding potential function.

The solution shows that the distortion consists in a simple cylindrical motion round the axis of rotation, each point moving from east to west with a linear velocity proportional to the cube of its distance from that axis.

The distortion of the surface of the globe consists of a motion in longitude from west to east, relatively to a point in the equator, the rate of change of longitude being proportional to the square of the sine of the latitude.

Numerical calculation shows, however, that in the later stages of the earth's history (the development being supposed to follow the laws found in the paper on "Precession"), the distortion must have been very small. With a certain assumed viscosity, it is found that, looking back 45,000,000 years, a point in latitude 60° would lie 14′ further east than at present. From this it follows, that this cause can have had little or nothing to do with the crumpling of geological strata.

As, however, the distorting force varies inversely as the sixth power of the moon's distance, it seems possible that in the very earliest stages this cause may have had sensible effects. It is, therefore, noteworthy that the wrinkles raised on the surface would run north and south in the equatorial regions, with a tendency towards north-east and south-west in the northern hemisphere, and north-west and south-east in the southern one. The intensity of the distorting force at the surface varies as the square of the cosine of the latitude.

An inspection of a map of the earth shows that the continents (or large wrinkles) conform more or less to this law. But Professor Schiapparelli's map of Mars* is more striking than that of the earth, when viewed by the light of this theory; but there are some objections to its application to the case of Mars. If, however, there is any truth in this, then it must be postulated, that after the wrinkles were formed the crust attained sufficient local rigidity to resist the obliteration of the wrinkles, whilst the mean figure of the earth adjusted itself to the ellipticity appropriate to the slackening diurnal rotation; also, it must be supposed that the general direction of the existing continents has lasted through geological history.

The second question, considered in the first part, deals with the

* "Memorie della Società degli Spettroscopisti Italiani," 1878, vol. vii.

non-rigid attachment of the permanent equatorial protuberance to the mean sphere. It is shown that the precessional and nutational couples will give rise to certain tides of the second order (varying as the tide-generating force multiplied by the precessional constant), but not to any secular shifting of the surface over the interior, as has been supposed would be the case by some writers.

II. *Distribution of Heat generated by Internal Friction, and the secular cooling of the Spheroid.*

In the paper on "Precession" it was shown by the theory of energy, that a very large amount of heat might have been generated inside the earth by friction, but the investigation gave no indication as to its distribution. The problem is here considered by finding the amount of work done per unit of time on each element of the interior in the course of the tidal distortion.

The aggregate work done on the whole globe is found to be the same as that given by simple considerations of energy. The rate of work is equal to the tidal frictional couple multiplied by the relative angular velocity of the moon and earth; but this simple law arises out of a complex law of internal distribution. By far the larger part of the work done, or heat generated, is found to be in the central portion.

My first impression was that the large amount of heat, which might be generated, would serve to explain in part the observed increase of underground temperature; but the solution of a certain problem concerning the cooling of an infinite slab of rock 8,000 miles thick, in which heat is being generated according to a certain law of distribution, shows that the frictional heat could not possibly explain a rate of increase of underground temperature near the earth's surface of more than 1° F. in 2,600 feet,

It follows, therefore, that Sir W. Thomson's investigation of the secular cooling of the earth cannot be sensibly affected by this cause.

III. *The Effects of Inertia in the Forced Oscillations of Viscous, Fluid, and Elastic Spheres.*

In the theory of tides used hitherto the effects of inertia have been neglected. It was, however, shown that this defect in the theory could not have an important influence, unless the frequency of the tides was much greater than that of those generated by the moon at the present time. Nevertheless it was desirable to determine what the effect of inertia actually is.

This part of the present paper contains a second approximation to the theory of tides of a viscous spheroid.

The first approximation, being that given in the paper on "Tides," is here used to give a value to the terms introduced in the equations of motion by inertia. Physically the terms so introduced are equivalent to an addition to the bodily force which tends to produce the tidal distortion. The problem is then treated by a process parallel to that used by Sir W. Thomson in his statical problem concerning the strain of an elastic sphere. The analytical investigation is long and complicated, and it will here suffice to state the result with regard to the form of the tidal protuberance, when the tide-generating potential is of the second order of harmonics. It is as follows:—If a be the radius, w the density, g mean gravity, and $\mathfrak{g}=\frac{2g}{5a}$, v the "speed" of the tide, η the alteration of phase; so that $\eta \div \mathrm{v}$ is the "lag," and υ the coefficient of viscosity.

Then $\eta-\frac{79\mathrm{v}^2}{150\mathfrak{g}}\sin\eta\cos\eta=\text{arc-tan}\,\frac{19\upsilon\mathrm{v}}{5\mathfrak{g}wa^2}$

And the height of tide is equal to the equilibrium tide of a perfectly fluid spheroid multiplied by—

$$\cos\eta\left(1+\frac{79\mathrm{v}^2}{150\mathfrak{g}}\right)$$

This shows that the defect of the first approximation was such that for a given speed, the lag is a little greater, and for a given lag, the height of tide is a little greater than was supposed.

It is then shown that this correction to the theory of tides will scarcely make any appreciable difference in the results of the integration, by which the secular changes in the configuration of the earth and moon, were found in the paper on "Precession;" and especially that it makes *no* difference as to the critical relationship between the month and day, for which the rate of change of obliquity vanishes. The most important influence of the new theory is on the time, and it appears that the time occupied by the changes, above referred to, is overstated by perhaps $\frac{1}{40}$th part.

A comparison is then made of the preceding theory with that of the forced vibrations of a fluid sphere. This shows that when η is zero (*i.e.*, when viscosity graduates into fluidity), the $\frac{79}{150}$ which occurs in the above expressions should properly be $\frac{1}{2}$ or $\frac{75}{150}$. The discrepancy between the 79 and 75 is explained by the fact that in approaching the problem of fluidity from the side of viscosity, we suppose in the first approximation, that the motion of the interior of the sphere is vertical, whereas in reality it is not so.

In conclusion, it is proved that analysis, of almost identically the same character as that for the problem of the viscous sphere, is applicable to the case of an incompressible elastic sphere, and that inertia has the effect of increasing the ellipticity of the tidal spheroid, as given

by Sir W. Thomson's statical theory, in the proportion of $1+\frac{79v^2}{150(r+g)}$ to unity, where v is the speed of the tide, and r is the quantity defined in Thomson and Tait's Nat. Phil., § 840 (28), viz., $\frac{19}{5wa^2}\times$ the coefficient of rigidity.

The last part of the paper contains a discussion of results, and a non-mathematical summary of what precedes.

IV. "On the Influence of Light upon Protoplasm." By ARTHUR DOWNES, M.D., and THOMAS P. BLUNT, M.A. Oxon. Communicated by J. MARSHALL, F.R.S., Surgeon to University College Hospital. Received October 9, 1878.

This paper is in continuation of, and supplementary to, a previous communication* in which we recorded the first part of an investigation on the effect of light upon *Bacteria* and other organisms associated with putrefaction and decay. The chief conclusions to which those observations led us were briefly as follow:—

(1.) Light is inimical to, and under favourable conditions may wholly prevent, the development of these organisms; its action on *Bacteria* being more energetic than upon the mycelial (and torulaceous) fungi which are prone to appear in cultivation-fluids.

(2.) The fitness of the cultivation-fluid as a nidus is not impaired by insolation.

We found also that tubes, containing a cultivation-fluid and plugged with cotton-wool, when removed to a dark place after exposure to the sun for a sufficient period, remained perfectly clear and free from organisms for months, and we naturally thought that the contents had been reduced to permanent sterility. The following facts, however, compel us to suspend for the present our conclusions on this point. Of the many tubes which we insolated last year we finally kept only three. Two of these—containing Pasteur solution of the composition given in our former paper—had been exposed to sunlight for three weeks in June, 1877; the third tube contained urine and had been insolated for about two months—commencing July 26th. In each case corresponding tubes which were covered with laminated lead, so as to exclude light, had swarmed with *Bacteria* in the course of two or three days, but the three tubes of which we speak not only were perfectly pellucid at the time they were removed from the light but, although kept in a warm room, remained clear all through the winter. On February 25th, 1878, however,—eight months after we had placed

* "Proc. Roy. Soc.," vol. xxvi, p. 488.

them in darkness—the two tubes of Pasteur solution each contained several tiny specks of mycelium.

One of the two was on this again exposed to sunlight, and in it the mycelial development was at once stopped; the other tube was left in the dark and the fungus gradually grew till it filled the whole space of the liquid, which on microscopical examination was found to contain no other organisms. The tube of urine remained clear till July 15th, 1878,—nearly ten months after incasement,—on which date two specks of mycelium appeared, and subsequently developed as in the previous case. No *Bacteria* could be seen on examination with an immersion $\frac{1}{16}''$. It is noteworthy that a companion tube to this which was incased after six days of insolation had developed a growth of mycelium in three or four days.

It would seem that in the three tubes above mentioned *Bacteria*, or their "germs," had been either wholly destroyed or reduced to so low a state of vitality that they were unable to develope in the fluids in question; while it is evident that the spores of the mould which at length appeared, unless they had been accidentally shaken down from the cotton-wool plugging the tubes, had undergone some change which reduced them to a condition of torpidity from which in process of time they emerged. Such a condition, we may perhaps conceive, might be brought about by any influence causing thickening of the cell-wall of the spore. We hope at a future time to offer some further evidence on this question of revival of dormant germs, which is, we think, of much interest.

From a very early period of our inquiry we have set ourselves to the task of investigating the intimate nature of the remarkable action of light upon these organisms, and we have arrived, as we believe, at a satisfactory solution of the problem, but in the first place it will be well to describe some preliminary experiments.

An interesting point to be determined was the question,—with what part of the spectrum is this property of light associated.

The observations made by us last year indicated that the rays of greatest refrangibility were the most active, but the experiments then made did not warrant any definite conclusions as to the part played by rays of lower refrangibility.

The method employed in the more recent experiments was similar to that described in our former paper:—

Small test-tubes containing the cultivation-fluid were suspended in deep narrow boxes made of garnet-red, yellow, blue, and ordinary glass respectively. Each box held about six test-tubes, and corresponding series were incased in laminated lead.

A spectroscopic examination of the glass of which these boxes were constructed showed that the yellow and blue were far from being monochromatic. The red was an excellent glass for the purpose.

The rays which were found to pass through each glass respectively are given below.

Blue.—Violet, blue, some green, broad band in yellow-green, very narrow band in ultra-red.

Yellow.—The whole spectrum, except violet and about half the blue.

Red.—Red, orange-red. All other rays entirely absorbed.

The mean of a number of observations as to temperature showed that, at the point at which we worked, viz., 70°—80° F., the thermometer in the red box stood about 2° F. higher than in the lead-incased tubes; between the blue, yellow, and ordinary glass boxes there was but little difference, the blue being about half a degree warmer than the last named.

We showed in our former communication that by increasing the density of our cultivation-liquid the development of *Bacteria* could be proportionately delayed. In this way we have been able to accentuate the differences in the behaviour of the solutions under varying conditions of light. Without detailing all the experiments, we may say that the first tubes to become turbid were the lead-incased; the next, usually in from 24—48 hours subsequently, the red, followed shortly by the yellow;* white and blue surviving.

The organisms which first appeared in the lead-incased and red were always *Bacteria;* in the yellow, usually *Torula*, or mycelium, with more or less *Bacteria*,—rarely *Bacteria* alone; if organisms appeared in the blue or ordinary glass they were torulaceous.

Although the blue and yellow glasses were not monochromatic, we think that these results give important indications. That the action is chiefly dependent on the blue and violet rays is shown by the great difference, as compared with those in the blue box, in the behaviour of the tubes in the yellow, in which, as we have already stated, the only rays of the spectrum not admitted were the violet and part of the blue.

Moreover, the fact that when the cultivation-fluid is of sufficient concentration the red (although the warmer) survives the lead-incased shows, we think, that the red and orange-red rays are not altogether inactive.

It is probable therefore that, if the phenomena were represented by a curve, the maximum elevation would be found in or near the violet, a rapid descent occurring in the blue or green, after which the line of the curve is maintained more or less as far as the visible red.

The experiments next to be detailed bear upon the part played by the cultivation-fluid in the phenomena under consideration. We had

* The only instance out of a large number of observations, in which yellow broke down before red, happens to be the experiment described in our former communication.

shown, in our previous paper, that the liquid in tubes which under insolation had remained barren was, nevertheless, not impaired as a nidus for development, for, on removing them to a dark place and inoculating with a drop of ordinary water, they soon teemed with vigorous bacterial life; the same experiment showing that the survival of the spores of mycelial fungi, as compared with *Bacteria*, was not due to any change in the cultivation-fluid rendering it noxious to the latter, but not to the former. At the same time, though this was not probable, there might have been a temporary and transient action dependent on some constituent of the cultivation-fluid. We determined, therefore, to render the conditions as simple as possible.

It is well known that all ordinary water, even distilled, teems with the "germs"—actual or potential—of various forms of life. We wished to ascertain whether or no sunlight would impair the vitality of, or destroy, "germs" existing in ordinary distilled water.

FIG. 1.

FIG. 2 (reduced).

Sealed ends of bulb bent at right angles to facilitate subsequent fracture.

We made a number of glass bulbs, of the shape shown in fig. 1, into each was introduced a measured quantity of a very concentrated

Pasteur solution, previously boiled; one end of each was then sealed. They were then placed in a water-bath, with the unsealed end projecting above the water, and after prolonged and repeated boiling this end also was sealed. The sealed bulbs were then thoroughly washed with distilled water, to remove all traces of Pasteur solution from their external surfaces, and were each finally sealed up in a tube (fig. 2) containing distilled water in such proportion that, when the bulbs were subsequently broken, the mixture produced a fluid of the ordinary strength. Four were incased in laminated lead and five insolated.

To prove that the water employed was capable of setting up bacterial or other development, a number of tubes containing Pasteur solution sterilised by repeated boiling and plugged with cotton wool were divided into two series; to each tube of the one set a few drops of the water were added with a superheated pipette; to the second series no water was added, but, in order to place them under the same conditions, the superheated pipette was successively dipped into each. All of the series inoculated with water speedily teemed with *Bacteria*; the second series remained clear.

The experiment commenced on April 3rd. About the end of May, the bulb in one of the insolated tubes was accidentally broken, so that the concentrated Pasteur solution of the bulb mingled with the distilled water of the tube. In a few days the mixture became turbid with *Bacteria* and *Torula*. We shall again refer to the behaviour of this tube.

The remaining bulbs were broken towards the close of July by jerking them against the ends of the tubes. The result was that, with one exception, the mixture in the tubes which had been insolated has remained clear to the date of writing (September 1st), but in each instance the incased tubes became turbid with organisms.

In the single insolated tube which broke down nothing could be seen on careful examination with $\frac{1}{12}''$ but round-celled *Torula*; there was a complete absence of all bacterioid life. The incased tubes all contained *Torula*, *Bacilli*, *Bacteria*, in active movement, and, in two instances, a number of short, squarish, highly refractive particles.

It is evident, therefore, that light is injurious to "germs," even when contained in ordinary distilled water. There is, however, an important fact in connexion with this which must not pass unnoticed. We have described how a tube in which the bulb had accidentally been broken after exposure to sunlight for six or seven weeks, in April and May, speedily teemed with *Bacteria*. It happens that, during portions of this time, we had insolated tubes, containing ordinary Pasteur solution, with the result that all bacterial development was prevented by a few days' exposure to the sun, and organisms, if they appeared after the tubes had been incased, were torulaceous or mycelial. There

appears, therefore, to be a remarkable difference in the rate of action of light on the germs of *Bacteria* in water, as compared with its effect on corresponding "germs" in the cultivation-fluid; insolation of, say a week, accomplishing, in the latter case, what nearly two months failed to do in the former.

The most reasonable explanation to our minds is the following:—In water destitute of organic matter the "germs" are deprived of the nourishment essential for their growth and development—they are starved; under these conditions their protoplasm reverts to a state of rest and stability, contrasting with that condition of instability which the exhibition of vital energy implies. Possibly they become encysted, the outer portion of the protoplasm being devitalised and protecting the central speck, which may be said to exist rather than to live. When, however, the "germ" finds suitable nourishment, the protoplasm takes on a higher state of activity and, therefore, of instability, and we believe that this instability of protoplasm favours the action upon it of light, but that in a condition of dormant vitality it is less susceptible.

Numerous other observations of similar character, which we need not here detail, gave the same results, and the following simple experiment, repeatedly confirmed, indicates the germicidal action of light when no water, other than the ordinary moisture in air, is present:—

April 15*th*. Four test-tubes are rinsed out with tap-water, inverted to allow the moisture to drain off, and plugged with cotton-wool. Two are covered with laminated lead, and two insolated in the usual way. (Corresponding tubes, charged with Pasteur solution previously sterilised by boiling, speedily became turbid with *Bacteria*.)

May 1*st*. The four tubes are charged with sterilised Pasteur solution.

In about a fortnight the lead-incased tubes both became turbid,* but the liquid in the insolated tubes was still clear on July 16th.

We now proceed to give an account of experiments which bear more directly on the intimate nature of the action under consideration.

From an early stage in the investigation we felt that the best way of approaching the problem was by examining the behaviour of organic bodies generally when exposed to sunlight. Taking, in the first instance, the comparatively simple molecule of oxalic acid as the subject of our experiment, we found that a decinormal solution (·63 per cent.) was entirely decomposed by sunlight. It was obviously important to ascertain what was the nature of the decomposition, *i.e.*, whether it were a *disintegration* of the molecule into water, carbonic acid, and

* Subsequent microscopical examination showed that this was due in the one case to a species of *Sarcina*, in the other to *Bacteria*, which did not, however, take on a very vigorous development.

carbonic oxide, or an *oxidation* resulting in water and carbonic acid alone.

We found that whether oxygen was removed by exhaustion at the Sprengel pump, or by boiling the solution and inverting in mercury without access of air, decomposition was alike prevented. It was evident, therefore, that *oxygen* was the agent of destruction under the influence of sunlight, for of course the nitrogen of the air may be put out of the question.

We next experimented on a representative of a most interesting class of bodies, which in the complexity of their composition probably approach protoplasm itself. We refer to the so-called soluble or indirect ferments, of which we selected *zymase*, the soluble ferment of yeast, as a type.

We noticed last year that sunlight had no retarding effect on the action of this class of ferments, but we did not then investigate the effect of prolonged insolation on the ferment itself. Accordingly, on June 25th, some water in which a fragment of yeast had been macerated was thrice passed through double layers of the finest filtering paper. Examined under the microscope, the liquid, which was quite clear, was found to contain no trace of *Torula*. Salt was then added to saturation, in order to avoid putrefaction, and the solution was divided between two series of test-tubes, one series being insolated, and the other incased in the usual way.

On July 19th about three drachms of freshly made syrup was placed in each of a number of *éprouvettes*. These were divided into two sets; to one set was added five grain-measures of the insolated *zymase* solution, and to the other a corresponding quantity from the incased tubes; a watch-glass was placed over each, and they were left for some hours. At the end of this time, five grain-measures of the syrup to which *zymase* from the incased tubes had been added completely reduced an equal quantity of a Fehling's solution, while no perceptible change was caused by the syrup which had been treated with insolated *zymase*.

It is clear, therefore, that sunlight destroys the specific power of this ferment for hydrating cane-sugar.

We next experimented on *zymase* in vacuo. On August 16th a solution of the ferment, prepared in the same way as before, was divided between eight tubes, two of which were insolated and two incased. The remaining four were simultaneously* exhausted at the Sprengel pump and sealed. The contents gave a sharp "water-hammer" click, bearing testimony to the excellence of the vacuum. Two of these tubes were insolated and two incased.

On September 5th, eight *éprouvettes* of fresh syrup were inoculated with liquid from each tube as before, and allowed to stand overnight, a

* See Appendix.

corresponding quantity of uninoculated syrup being kept to ascertain if any hydration occurred spontaneously.

September 6th.—The uninoculated syrup has no perceptible reducing action on Fehling's solution, but the contents of all the vacuum-tubes, whether insolated or incased, have produced hydration of the cane-sugar, and there is no practical difference between their effect—as measured by Fehling's solution—and that of the non-Sprengelised *zymase* preserved in the dark. At the same time the corresponding solutions of *zymase* insolated without previous exhaustion have very feeble action indeed. We conclude, therefore, that, as in the case of the oxalic acid, the destructive action of light is by oxidation, for, while the *zymase* exposed to light and air was greatly enfeebled, a similar solution in vacuo, although equally insolated, retained its energy apparently unimpaired.*

In proceeding to investigate the nature of the action of light upon living organisms, we were met by difficulties, arising from the relation of these organisms to oxygen, which for some time baffled our research; but these difficulties we have, we believe, sufficiently overcome to be enabled to indicate the fundamental identity of the action of light upon living organisms and upon the typical non-vitalised organic substances selected for our previous experiments.

In a postscript appended to our previous communication we stated that in sealed tubes containing urine, which had been exhausted at a Sprengel pump, organisms appeared in incased and insolated tubes alike.

We have made many repetitions of these experiments, and we have invariably found that, whenever organisms appeared in the incased exhausted tubes, they were simultaneously present in equal amount and vigour in the insolated, contrasting with the difference in behaviour between corresponding insolated and incased non-exhausted tubes.

It seemed, therefore, that in absence of an atmosphere, light (notwithstanding the manifest enfeeblement of life brought about by the

* It was our original intention to examine a large number of organic bodies, and to ascertain to what extent this phenomenon of oxidation under sunlight occurred in different classes of organic compounds. The recent researches of M. Chastaing ("Ann. de Chim. et de Phys.," [5], t. xi) have anticipated us in this. M. Chastaing experimented on such organic compounds as essence of turpentine, essence of lemon, ether, oils, &c., all of which were oxidised in sunlight, the oxidation occurring in all parts of the visible spectrum, but having a maximum in violet and a minimum in red. It is noteworthy to observe how this distribution of the function of oxidation of these substances in the spectrum, according to M. Chastaing, corresponds with that assigned by ourselves on entirely independent grounds to the destructive action of light on *Bacteria*. We should have stated, also, that, according to our experiments, the oxidation of oxalic acid was very active behind blue glass, but feeble behind red.

withdrawal of air) failed entirely to produce any effect on such organisms as were able to appear.*

Experiments in which nitrogen was admitted into the exhausted tubes before they were sealed gave similar results.

The obvious inference that the presence of oxygen is essential to this action of light is confirmed by the following experiment—many times repeated—showing that the effect is in direct relation to the proportion of free oxygen :—

A Pasteur solution of half the strength given in our former paper, and therefore, for reasons stated in that paper, difficult to sterilise by insolation, was divided between six tubes.

Two of these were simply sealed, and therefore contained an atmosphere of ordinary air.

Two were exhausted at the Sprengel pump till the gauge stood at a height of 22 inches, when, by means of the apparatus described in the Appendix, nitrogen was admitted, and the tubes being sealed, consequently only contained about one-twentieth of oxygen in their atmospheres. The remaining two tubes were exhausted thoroughly and pure oxygen admitted in the same manner as the nitrogen.

One tube of each series was incased in laminated lead, the companion tube being insolated.

In two days all the incased tubes were equally turbid with *Bacteria.*

In two days more the insolated tube, with $\frac{1}{20}$th oxygen atmosphere, was turbid with *Torula* and *Bacteria.*

Next day after this, the insolated tube, with an atmosphere of ordinary air, became hazy with *Bacteria.*

The tube, with an atmosphere of pure oxygen, remained unchanged for some days later, when a deposit of *Torula* commenced to form at the bottom.

We conclude, therefore, both from analogy and from direct experiment, that the observed action on these organisms is not dependent on light *per se*, but that the presence of free oxygen is necessary ; light and oxygen together accomplishing what neither can do alone: and the inference seems irresistible that the effect produced is a gradual oxidation of the constituent protoplasm of these organisms, and that, in this respect, protoplasm, although living, is not exempt from laws which appear to govern the relations of light and oxygen to forms of matter less highly endowed.† A force, which is indirectly absolutely

* The commonest form of organism in these exhausted tubes consisted of filaments of varying length, ranging perhaps from $\frac{1}{5000}''$ to $\frac{1}{500}''$, often curvilinear, composed of minute spherules in linear series, with motion usually vibratory and undulating, frequently progressive.

† That the amount of free oxygen present need not be large to produce a definite action upon *Bacteria* is shown by the fact that tubes containing Pasteur solution with a supernatant layer of vaseline, excluding all air, except that previously dissolved in the solution, if encased, in a few days become turbid, but may be kept clear for

essential to life as we know it, and matter, in the absence of which life has not yet been proved to exist, here unite for its destruction.

The organisms (*Bacteria*) on which we have mainly experimented, in their ordinary conditions of structure and development, afford an example of protoplasm in a simple and uncomplicated form, but it would be unreasonable to suppose that this protoplasm is so essentially different in its fundamental constitution from all other protoplasm that here, and here only, is this special effect of light to be found. There are, indeed, many facts which prove the contrary, and indicate that we are dealing, not with a special and fortuitous phenomenon, but with a general law.

But protoplasm may be very differently circumstanced in its relations both to light and oxygen, it may be protected. Such protection may be afforded by :—

1. Thickened, or opaque, cell-walls or envelopes.
2. Special colouring matters, which filter out the more injurious rays.
3. Aggregation of cells, whether free or combined into tissue, the inner being protected by the external.
4. Relation of the protoplasm itself to oxygen.

The first three are sufficiently obvious, but, as regards the last-named condition of protection, a few words of explanation are necessary.

Protoplasm in its relation to oxygen varies widely. In the vast majority of cases, oxygen in its free gaseous state, appears to be absolutely essential for the development and reproduction of protoplasmic life, but the labours of Pasteur have sufficiently demonstrated the power of some organisms, living in absence of free oxygen, to take it from certain of its combinations. During the present summer we have been continually troubled in our investigation by the fact, that either our materials, or the air in which we worked, had become infected with a species of small *Torula*. Solutions exposed to sunlight would remain clear for a few days—their incased companions in the meantime becoming turbid with ordinary *Bacteria*—but slowly and gradually a deposit would form at the bottom of the solution, which, on examination, would prove to be the *Torula* in question.

Now, if we consider the rapidity with which *Torula* removes dissolved oxygen from water,* and the comparative slowness with which

a considerable period by insolation. This fact, as well as others, shows, by the way, that the action does not depend on ozone formed, as Gorup v. Besanez believes ("Ann. Chem. Pharm.," clxi, 232) is invariably the case when water evaporates. We have, we may observe, never been able to detect the formation of active oxygen as ozone, or peroxide of hydrogen, in cultivation solutions or in water exposed to sunlight.

* Schützenberger, "Fermentation," pp. 107 and 134.

water dissolves that gas, we shall at once see that the *Torula*, deriving its respiratory oxygen from the sugar of the solution, is all this time living comparatively in absence of free oxygen, and we understand how the relations of protoplasm to oxygen, by enabling it in some forms to be largely independent of the uncombined gas, may prove a source of protection against the oxidising action of light.

In some cases, indeed, the affinity of organisms for oxygen would appear to be so great that, when presented to them in its gaseous and uncombined state, it acts, not as a source of vital energy, but as a poison, and we think that protoplasm will be found to possess varying degrees of tolerance of excess or deficiency of this element. To some forms of life, if Pasteur be right, oxygen is injurious even when diluted as in ordinary air, to others it is hurtful only when oxidation is quickened by some adjuvant force, as, for example, by light. Finally, since light here acts as an oxidiser, it is conceivable that there may exist sluggish forms of protoplasm, whose oxidising processes, and, therefore, general growth and development, may be *favourably* augmented by a modified degree of light. We are not of our present knowledge, however, able to point to such.*

In connexion with the subject of this paper, it is an interesting speculation whether any one of the constituent elements of organic bodies is specially subject to oxidation under light. We seem to have obtained some glimpse of a possible answer to this question by a few experiments upon the oxalates. If the constitutional formula of oxalic acid be rightly represented thus, $\begin{array}{l} \mathrm{C—O—O—H} \\ \,||| \\ \mathrm{C—O—O—H} \end{array}$, a mode of approach to the problem seems to be opened, for should we find on substituting some other element for the hydrogen that decomposition is no longer produced by light, the conclusion would seem inevitable that the destruction of the molecule of oxalic acid was effected through the oxidation of the hydrogen.

On July 26th a solution of neutral oxalate of potash of decinormal strength was divided between a number of test-tubes, some of which we incased while some were insolated in the usual way. At the same time a decinormal solution of oxalic acid was similarly treated.

August 26*th*. The insolated oxalic acid solution is completely decomposed, but the incased oxalic acid solution is unaffected. The solutions of oxalate of potash, both incased and insolated, remain quite unchanged, and are still neutral to test-paper.

* From what we have said, it would follow that the organisms most injuriously affected by light would be found to be those whose protoplasm is "unprotected," having high affinities for oxygen, but yet for the most part requiring it uncombined, and at the same time being so minutely particulate as to offer in point of surface the greatest facility for access both of light and of oxygen, all of which conditions are exemplified by the ordinary forms of *Bacteria*.

We are justified, therefore, in concluding that, in this case at least, the destruction of an organic body in light is due to the oxidation of its hydrogen.

Appendix.

Vacuum Apparatus.—In conducting our experiments in vacuo, or in a modified atmosphere, obtained by means of the Sprengel pump, it was necessary to ensure that the tubes compared should be under exactly similar conditions with regard to pressure; and it seemed desirable, therefore, to exhaust the pairs, or double pairs, at one operation. With this object an *adapter* was contrived, which, though simple in its construction, proved so efficient that it may be worth while to describe it in detail.

Fig. 3.—*a*, "shoulder." *b*, the point of sealing after exhaustion.

A piece of glass tubing, 1½ inches long, ¾ inch in diameter, and open at both ends, which were slightly lipped, was fitted with two caoutchouc stoppers, one of which, pierced with a single hole, served for connexion

with the entrance tube of the pump, while the other had bored in it four holes into which the ends of the experimental tubes were pushed until the "shoulder" (see fig. 3) was firmly thrust against the india-rubber. All junctions were luted with viscid glycerine, and it was found that a good vacuum could then be produced and maintained for a considerable time.

In order conveniently to seal off the tubes they were again drawn out below the shoulder, so that when complete they had the shape given in the figure (fig. 3).

When atmospheres of special composition were required the mode of procedure was somewhat different; one of the four holes in the outer caoutchouc stopper was then appropriated to a gauge, formed of a straight piece of tubing of sufficient length, dipping under mercury: into another hole was fitted a glass tube to which was attached a piece of india-rubber tubing with a clamp. The pump was then worked until the gauge showed the required tension, when the gas was admitted from a small gasholder by attaching the stop-cock of the gasholder to the india-rubber tubing and opening the clamp.

The *nitrogen* used was prepared by removing the oxygen from atmospheric air, either by the prolonged action of alkaline solution of pyrogallic acid, or, in some instances, by the combustion of phosphorus; in the latter case the oxides of phosphorus were removed by agitating with solution of caustic potash.

Our *oxygen* was made by heating pure chlorate of potash alone in a tube of hard glass; lest any trace of ozone or chlorine should be present the gas was slowly bubbled through solution of iodide of potash; this precaution, however, appeared to be superfluous, the iodide solution remaining colourless.

Postscript. Received October 18, 1878.

The oxidation of hydrogen by light, demonstrated in the case of oxalic acid, naturally suggests an inquiry into the deportment of oxygen towards hydrogen in sunlight under other conditions.

We have not, for the present at least, an opportunity of examining this question in the detail which it demands, but we think that it may be of interest to append to our paper the following brief observations.

One of the best known facts in the chemistry of light is the combination effected between chlorine and hydrogen, and in their behaviour towards hydrogen under the influence of light the halogens form an interesting series. Thus, while chlorine and hydrogen unite explosively in sunlight, bromine and hydrogen are with difficulty, if at all, induced to combine, and iodine and hydrogen do not unite at all. Again, water may be decomposed with the aid of sunlight both by chlorine* and by

* $Cl_2 + H_2O = 2HCl + O$.

bromine,* but not by iodine. Finally, while hydrochloric and hydrobromic acid in aqueous solution each resist decomposition when insolated in the presence of free oxygen, it is known that hydriodic acid under like conditions is rapidly destroyed.† This destruction, according to our experiments, is promoted by all the rays, but is much less active behind red glass than behind blue. It occurs also, but more slowly, in the dark.

Here we appear to have a phenomenon analogous to the oxidation of the hydrogen of oxalic acid.

The question arises how far a preliminary dissociation of the constituent atoms of the molecule may influence the reaction. It has been clearly shown by M. Lemoine‡ that hydriodic acid *gas* is completely dissociated by light; but the same observer states that in aqueous solution no such dissociation in sunlight can be demonstrated—a fact observed also by M. Berthelot. It may be, however, that the phenomena of dissociation and oxidation under light may go on side by side, the presence of oxygen promoting the splitting of hydriodic acid by its determining affinity. In like manner it may be that in the decomposition of oxalic acid the oxygen plays a similar part, determining the dissociation of $C_2O_4.H_2$, and replacing the dissociated radicle C_2O_4. The analogy of chlorine, however, leads us to the belief that, in its relations to hydrogen under the influence of light, oxygen may be classed with that element; but the reactions above noted would seem to indicate that, under these conditions, its affinity for hydrogen is inferior to that of either chlorine or bromine.§

We would note also the following known reactions which occur in air and sunlight:

(1.) The decomposition of arsenamine with formation of water and deposition of arsenic.

(2.) The absorption of oxygen by and precipitation of sulphur from sulphuretted hydrogen;—reactions which, although occurring in the dark, are accelerated by sunlight.

V. "Note on the Influence exercised by Light on Organic Infusions." By JOHN TYNDALL, D.C.L., F.R.S., Professor of Natural Philosophy in the Royal Institution. Received December 17, 1878.

Early last June I took with me to the Alps 50 small hermetically sealed flasks containing infusion of cucumber, and 50 containing

* $Br_2 + H_2O = 2HBr + O$.

† $2HI + O = I_2 + H_2O$.

‡ "Annales de Chim. et de Phys.," [5], t. xi.

§ Under ordinary conditions the direct combination of oxygen and hydrogen gases does not occur in sunlight.

turnip infusion. Before sealing they had been boiled for five minutes in the laboratory of the Royal Institution. They were carefully packed in sawdust, but when unpacked the fragile sealed ends of about 20 of them were found broken off. Some of these injured flasks were empty, while others still retained their liquids. The 80 unbroken flasks were found pellucid, and they continued so throughout the summer. All the broken ones, on the other hand, which had retained their liquids, were turbid with organisms.

Shaking up the sawdust, which I knew must contain a considerable quantity of germinal matter, I snipped off the ends of a number of flasks in the air above the sawdust. Exposed to a temperature of 70° or 80° F., the contents of all these flasks became turbid in two or three days.

The experiment was repeated; and after the contaminated air had entered them, I exposed the flasks to strong sunshine for a whole summer's day; one batch, indeed, was thus exposed for several successive days. Placed in a room with a temperature of from 70° to 80° F., they all, without exception, became turbid with organisms.

Another batch of flasks, after having their sealed ends broken off, was infected by the water of a cascade derived from the melting of the mountain snows. They were afterwards exposed to a day's strong sunshine, and subsequently removed to the warm room. In three days they were thickly charged with organisms.

On the same day a number of flasks had their ends snipped off in the open air beside the cascade. They remained for weeks transparent, and doubtless continue so to the present hour.

I do not wish to offer these results as antagonistic to those so clearly described by Dr. Arthur Downes and Mr. Thomas Blunt, in the "Proceedings of the Royal Society," for December 6th, 1877.* Their observations are so definite that it is hardly possible to doubt their accuracy. But they noticed anomalies which it is desirable to clear up. On the 10th of July, for example, they found 9 hours' exposure to daylight, $3\frac{1}{2}$ hours of which only were hours of sunshine, sufficient to effect sterilization; while, on the 29th of July, "a very hot day, with much sunshine," 11 hours' exposure, "9 of which were true insolation," failed to produce the same effect. Such irregularities, coupled with the results above recorded, will, I trust, induce them to repeat their experiments, with the view of determining the true limits of the important action which those experiments reveal.

* Vol. xxvi, p. 488.

VI. "On the Structure and Development of the Skull in the *Lacertilia*. Part I. On the Skull of the Common Lizards (*Lacerta agilis*, *L. viridis*, and *Zootoca vivipara*)." By W. K. PARKER, F.R.S. Received October 18, 1878.

(Abstract.)

The youngest, and therefore the most important, embryos that have been worked out in this present piece of research, were sent me, with those of the snake, by Dr. Max Braun, of Würzburg.

Other valuable specimens were the gifts of Professor T. Rupert Jones, F.R.S., and Professor Alfred H. Garrod, F.R.S.

The *three species* worked out are closely related, and two of them are native to this country: these familiar *Sand Lizards* are amongst the smallest, and yet the most highly specialized, types, to be found among the Reptilia.

This type may be taken as a sort of "norma," and by it all the other Lacertilia may be measured, as it were, when their height in the Reptilian scale is to be determined.

When such forms as *Hatteria* and the chamæleon are compared with a typical Lacertian, then we see how much there is that is generalized in those outlying species.

Putting together what I have learned as yet of the structure of the skull in the true Reptiles, and comparing what is seen in these cold-blooded Sauropsida with what is seen in the hot-blooded bird, I have come to the conclusion that the common lizard is a *culminating type*.

The snake, the tortoise, and the crocodile, notwithstanding their own peculiar specializations, are yet more *general* in their nature than the nobler and higher kinds of lizards: this is especially shown by the number of characters that are, in the latter, in conformity with those of the bird.

And, indeed, with the *high* or Carinate bird; for the skull of the Ratitæ (ostrich and cassowary) does not undergo, in several things, so much metamorphosis as the skull of the typical lizard; for, as I showed long ago, these birds are not devoid of a *Batrachian* strain.

Of all the lizards known to me the chamæleon is the lowest; in some respects the Chelonians come nearer the higher Lacertilia than that *bizarre* type does. I have carefully worked out the skull in the adult and the ripe embryo of the common kind, and in the adult of the *dwarf* species.

In several things the lizard's skull is but little modified from that of the snake; this is especially seen in the nasal structure, its glands, and the bones of its floor; so largely illustrated in my last paper.

These things are not repeated in the Chelonia and crocodiles, nor do they exist in the chamæleon; but in many birds, especially the

"songsters," these curious specializations reappear, but the parts are lessened and modified.

Even many of those metamorphoses of the skull, which when I worked out that of the chick seemed to me to be peculiarly *avian*, and indeed not to be found amongst the almost *reptilian* Ratitæ, now turn out to be *lacertian* also.

For instance, the separate cartilages that pad the "basi-pterygoid processes" of the skull and the pterygoid bones, at their articulation, these appear in the lizard; and even the division of the *septum nasi* from the ethmoidal wall begins in *Lacerta*, and other lizards.

That separation of the two regions has its explanation in the higher birds, whose fore face hinges on the skull; notably in the parrot.

In *Lacerta* it is a mere "fenestra," of no use to the creature; so it is in the semi-struthious Tinamou, and in some low, Southern passerine birds, *e.g.*, *Grallaria squamigera*.

But in the huge Ratitæ it is as absent, as in the Chelonia, and the low chamæleon.

This latter kind has no column-shaped bone on the pterygoid ("epi-pterygoid"); that bone exists but is small and modified in the Chelonia; in birds, especially the "*songsters*," it is manifestly a *process* of the pterygoid, but I have never seen it as a distinct bone.

These are some of the more striking characters in the skull of the adult lizard and its *sauropsidan* relatives, namely, snakes, tortoises, crocodiles, and birds: the latter, it may be remarked, differ less in their structure from a lizard than many an imago-insect does from its pupa.

I have a strong suspicion that the serpent is degraded as well as more ancient and generalized, as compared to the lizard: it has manifestly lost its limbs, and the correlate of that loss is an arrest of the cartilaginous cranium. The small rudiments of orbitosphenoids and alisphenoids, seen in the snake, are no longer an anomaly and unexplainable: they are patches of the large tracts in the lizard, which has, contrary to what I long believed, a large alisphenoid on each side.

This part is not a continuous flap of cartilage: in the bird it is, but it always has a great fenestra in its middle, even in them; in the lizard it is multi-fenestrate—a mere basket-work of cartilage, feebly and partially ossified.

In its auditory structures the high Lacertian corresponds very closely with the tortoise and the crocodile, and these three kinds differ only in non-essentials from the bird.

The snake and the chamæleon lie below them all, but the chamæleon is lower than the snake, and has a worse ear than most frogs and toads. The lower jaw of the lizard and the nestling bird agree very closely. The remains of the hyoid and branchial arches are far more ichthyic in the lizard than in the bird.

From familiar things I pass to things little known; that is, to the early stages of the lizard.

In the early stages I cannot confine myself to the nerve-supporting organs, but, of set purpose, let my work overlap that of my friend Mr. Balfour, who is, to me, the typical embryologist; Mr. Milnes Marshall's excellent papers, however, are not forgotten.

Much that is figured of the earlier stages is not described; my illustrations can, however, easily be compared with those of the *chick* in Foster and Balfour's work; and with the copious and exquisite illustrations given in Mr. Balfour's work on the "Elasmobranchs."

The reader is asked to refer to these works, especially the latter; that he may see how perfectly my observations on the embryo of the lizard correspond with what Mr. Balfour has discovered in other types.

Some of the most important of them relate to structures that must be well understood before we can gain even the most elementary conceptions of the morphology of the vertebrate skeleton.

These are—the brain and main nerves; the sense-capsules; the respiratory openings (clefts) through the wall of the throat; the "pituitary body," and its relation to the mouth and brain; and the extension into and subdivision of of the pleuro-peritoneal cavity in the head, even in front of the mouth.

The modification of the "segmental" muscular masses in the head; the difference between the axial structures of the head and the body; all these things have to be carefully attended to.

I will now propound my own theory of the skeleton of the head and throat, as compared with the skeleton of the body generally, namely, the spine and thoracico-abdominal cavity.

The undivided condition of the paired tracts, on each side of the notochord, which is so constant in the head, is the original state of things; the head is archaic, the trunk, with its vertebræ intercalating with the muscle-plates, is a much more modern result of evolutional metamorphosis than the undivided head; the limb-girdles and limbs are the newest of all.

Archaic entomocranial Vertebrates, had no vertebræ, properly speaking; they had a long head, composed of fourteen or fifteen segments; their throat was a large multiperforate bag; and instead of having one vagus nerve, they had seven or eight pairs of vagi, forking over all the respiratory passages, except those supplied by the glosso-pharyngeal and portio dura.

Some of them were like Cæcilians; they had long, vermiform bodies, and scarcely any tail behind their anal opening; they had no finished vertebræ, but a semi-solid, half-cartilaginous tube, surrounding the notochord.

Others were a sort of exaggerated tadpoles; they were the fathers

of all such as gradually improved into the larval condition (for a long while permanent) of the modern Batrachia, but they were *Ametabolous*, or arrested.

These ancient bull-heads had a huge pharynx, under which, more than behind, a very short abdomen was swung, with a snake-coiled intestine; their body was a mere lash, like the lash on the tail of the larva of the smooth newt and *Dactylethra*, and the lash of the tail of the adult *Chimæra*.

The forms from which the Marsipobranchii on the one hand, and the *Chimæra* on the other, sprung, were intermediate between the two extreme forms imagined; they were, however, close akin to the primordial tadpole.

What the pituitary body was, at that time, when the mesocephalic flexure just appeared; how the vesiculation of the neural axis arose; and whether the sense-capsules were at first paired or unpaired; of these things I will speak when I have obtained more light upon this dark subject.

But, even in the foggy illumination of the present, we can make out that even the term "the vertebral theory of the skull," is absurd; vertebræ, as such, are a late specialization of a segmented creature, whose mouth is opposite its nervous axis, and on the same aspect as its main circulating organ (hæmostomous).

For a long while there was no definite division into head and body; the Selachians show this to this day; their investing mass or parachordal tracts run on from the head into the body without division: the occipito-atlantal articulation is very late in its appearance.

Moreover, both the lamprey and *Heptanchus* show (or indicate) that the head of modern Vertebrates has been greatly shortened—much more than their throat; the cervical vertebræ are new segments of the axis, intercalated at that part, to bind the shortening head to the retreating body.

This view is curiously strengthened by an observation of Mr. Balfour's, with regard to the formation of "somatomes" in the cervical region of the chick; the foremost do not appear first, but the 4th, 5th, 6th, &c., are to be seen first, and then the three front segments.

Dr. Milnes Marshall's observations on the segmental nerves of the chick,* showing that the third, or *motor oculi*, is as good a segmental nerve as the great 5th, or trigeminal, and that the olfactory or first nerve is developed exactly in the same manner as the other cranial nerves, namely, from the dorsal region of the "epiblast;" these discoveries, I think, are of the greatest importance, and are very suggestive.

* See "Quarterly Journal of Microscopical Science," vol. xviii, New Series, Plates 2, 3, pp. 1—31.

Even those who are content to work at the development of the lowlier types, such as the worm and the cray-fish, are helping at this good work, for they are throwing light upon the evolution of the Vertebrates.

VII. "On the Chemical Composition of Aleurone Grains." By SYDNEY H. VINES, B.A., B.Sc., F.L.S., Fellow and Lecturer of Christ's College, Cambridge. Communicated by Dr. MICHAEL FOSTER, Prælector of Physiology in Trinity College, Cambridge. Received October 22, 1878.

I. *The Aleurone Grains of the Blue Lupin.* (*Lupinus varius.*)

The proteids stored up in the seeds of certain plants, more especially of Leguminosæ, have been stated by various observers to exist in the form of the vegetable caseins such as Legumin and Conglutin, and this view has been advocated of late years more particularly by Ritthausen ("Die Eiweiss-Körper der Getreidearten, &c., 1872"). In 1877, Weyl published some observations ("Zeitschr. für Physiol. Chemie, Bd. I), which tend to show that the proteids exist in the seeds of these plants in the form of globulins, and that the caseins, extracted by Ritthausen and others, are the products of the alteration of the globulins effected by the reagents (alkaline solutions) used in their extraction.

In order to be in a position to form a decided opinion upon the subject, I first repeated Weyl's experiments, using the seeds of the blue lupin. I found that on treating the ground seeds with 10 per cent. NaCl solution, I obtained a fluid which gave all the reactions characteristic of fluids which hold globulins in solution. On dilution with water it gave a precipitate of a substance soluble in 10 per cent. NaCl solution (vitellin); and on saturating it with NaCl (rock-salt), a substance (myosin) was precipitated which was soluble in 10 per cent, NaCl solution.

With the view of ascertaining the value of Weyl's suggestion, that the casein (conglutin, Ritthausen) contained in the lupin was a product of the alteration of the globulin under the action of an alkaline solution, I made the following experiment: — About 50 grms. of the ground lupin-seeds were placed on a filter, and 250 cub. centims. 0·1 per cent. NaHO solution poured over them. The fluid ran through in a few minutes, and was found to give the reactions characteristic of alkaline solutions of vegetable casein (*see* "Sachsse, Chemie und Physiologie der Farbstoffe," &c., 1877, p. 267). The residue on the filter was then well washed with distilled water until the washings ceased to give an alkaline reaction. It was then treated with 250 cub. centims. 10 per cent. NaCl solution, and on testing the filtrate it was

found to hold much globulin in solution. The residue on the filter was then placed in a beaker with 500 cub. centims. of the 0·1 per cent. NaHO solution, and allowed to stand for twenty-four hours. At the end of that time the alkaline fluid was poured off, and the residue placed on a filter and well washed with distilled water. On treating it with 10 per cent. NaCl solution it was impossible to extract from it more than the merest traces of globulin. It appears, therefore, that the globulin had become altered by the action of the alkaline fluid, that it had in fact become dissolved in it in the form of alkali-albumin. This change probably occurs in the extraction of conglutin by Ritthausen's method.

Moreover, I found that conglutin prepared according to Ritthausen's methods gives reactions which are characteristic of the substances formed when various animal proteids are treated with dilute acid or alkaline solutions (acid-albumin, alkali-albumin), and it does not differ very widely from these substances in elementary composition. These facts support the view that conglutin is merely a product of the alteration of the true reserve-proteids. Weyl had already shown that no proteids, except such as are soluble in 10 per cent. NaCl solution, can be extracted from the seeds by treating them with 1 per cent. Na_2CO_3 solution. This proves that conglutin does not pre-exist in the seed.

I therefore agree with Weyl in concluding that the proteids stored up in the seeds of the blue lupin consist of globulins (vegetable vitellin and vegetable myosin).

Subsequent observations, however, assured me that this is not the only form in which the reserve-proteids are present. I found that the 10 per cent. NaCl extract of the seeds contained, in addition to the globulins, a proteid in solution, which was not precipitated by boiling, or by saturation with rock-salt, or by dilution with distilled water. This substance may be isolated by extracting the ground seeds with distilled water; boiling the extract several times to remove all traces of globulin; evaporating to small bulk over a water-bath, and allowing the fluid to filter into absolute alcohol. As it drops into the alcohol a dense precipitate is formed. The substance which is thus precipitated is readily soluble in distilled water even after being exposed for months to the action of alcohol. Its solution in distilled water does not become turbid on boiling; it gives a precipitate on the addition of a drop of HNO_3, which is soluble in excess of acid; it gives the xanthoproteic and Millon's reactions; it gives an immediate precipitate with acetic acid and potassic ferrocyanide; and it gives a bright pink colour when treated with excess of strong NaHO solution on the addition of a drop of dilute $CuSO_4$ solution. The substance does not dialyse. These properties and reactions indicate that the substance is allied to the peptones. It most nearly resembles the *a* peptone of Meisoner, or,

adopting Kühne's nomenclature ("Verhandl d. Nat.-Med. Vereins zu Heidelberg," Band I, 1876), the substance to which he gives the name of Hemialbumose; a name which may be provisionally applied to this substance also.

The proteids stored up in the seeds of the blue lupin are therefore of two kinds:

(1.) Hemialbumose—soluble in distilled water.

(2.) Globulins—insoluble in distilled water, but soluble in 10 per cent. NaCl solution.

In order to determine the exact distribution of these substances in the cells of the seed, I made a series of micro-chemical observations. Thin sections of the cotyledons were placed for a few minutes in ether and then in absolute alcohol, in order to remove the fatty matters present which would otherwise interfere with the observation. A section examined in a drop of absolute alcohol shows the cells filled with aleurone grains lying in the meshes of a delicate matrix. They are hyaline or faintly granular, and have a yellowish tint. On adding a few drops of distilled water the grains become coarsely granular; the granules gradually disappear, and then vacuoles make their appearance. Further treatment with water produces no apparent change. If now a few drops of 10 per cent. NaCl solution be added, the hyaline vacuolated grains at once disappear, and nothing remains in the cells (when the section is very delicate) but the network of the matrix. A precipitate may be produced in the fluid under the cover-slip by diluting it with distilled water. The precipitate assumes the form of rounded drops of a viscous nature which are readily redissolved on the addition of NaCl (vegetable vitellin). If the section be irrigated with 10 per cent. NaCl solution until the addition of distilled water produces no precipitate, and if it be then well washed with distilled water nothing remains within the cells but the matrix. This is rendered conspicuous by adding a drop of solution of iodine which gives it a bright yellow colour.

It is well known that aleurone grains consist essentially of proteids, but the nature of these proteids has not as yet been determined. From the foregoing observations it appears that at least one proteid is present which is soluble in water, and one which is insoluble in water but soluble in 10 per cent. NaCl solution. The preceding chemical experiments suffice to prove that the former is hemialbumose, and that the latter includes the two forms of vegetable globulin.

My observations on the solubility of the aleurone grains of the blue lupin in water agree in the main with those of Pfeffer ("Unters. über Proteïn-Körner, &c. Jahrb. f. Wiss. Bot.," Band VIII, 1872, p. 447), but I have been unable to discover that, as he asserts in the case of Pæonia and Cynoglossum at least, long continued exposure to alcohol diminishes their solubility in water. Such treatment affects neither

the solubility of the hemialbumose in water, nor that of the globulins in 10 per cent. NaCl solution, but it renders the protoplasmic matrix of the cells quite insoluble in dilute alkaline solutions. These facts were established by experiments with grains which had been in alcohol for three months.

I have detected the presence of hemialbumose in the seeds of vetches and of the hemp and flax plants, and I propose to study the mode of its occurrence in the seeds of these and other plants, as I have already done in the case of the blue lupin, and further, to determine what is its exact significance in the process of germination.

VIII. "Report on Phyto-Palæontological Investigations generally and on those relating to the Eocene Flora of Great Britain in particular." By Dr. CONSTANTIN BARON ETTINGSHAUSEN, Professor in the University of Graz, Austria. Communicated by Professor HUXLEY, Sec. R.S. Received December 12, 1878.

When, about thirty years ago, I began to direct my attention to the study of the fossil Flora, the knowledge of fossil forms of plants was confined almost exclusively to forms of the Palæozoic formations. Of the Tertiary Flora there existed at that time a very imperfect conception; but few beds of Tertiary plants were known, and these had been only superficially examined. Leaf-skeletons had not been examined, and consequently the characteristic marks upon them were not available for the purpose of instituting a comparison with the fossil leaves. The fossils themselves were only obtained from stones which had been exposed to the air, and were easily split asunder, and it was thus impossible to arrive at any accurate knowledge of the nature of the old world plants. In fact, parts of one and the same plant were often regarded as plants of different genera. Thus on making a closer and more careful investigation into the Coal Flora of Bohemia, I was able to show that the *Asterophyllites* are the branches, and the *Volkmanniæ* the fruits of the Calamites.

It appeared to me, therefore, necessary that I should devote myself to the study of the so-much-neglected Flora of the Cainozoic formations. With this object in view, I determined:—

Firstly, to collect fossil plants as completely as possible, in order that my investigation should produce results on which I might entirely rely.

Secondly, to improve the method of investigation, especially with regard to the working out of the skeletons of the leaves of living plants, so as in that way to acquire sure standpoints from which to determine the species of the fossil leaves.

Thirdly, not to confine the scope of the inquiry within the limits of

mere palæontological interest, but above all to extend it to the unveiling of the history of the development of the whole vegetable kingdom.

As in studying the Eocene Flora of Great Britain I shall follow the path of the inquiry which I originally took, I must begin by giving an account of my method of investigating fossil plants, and I shall then explain the results which I have obtained.

I.—*The Method of obtaining Fossil Plants.*

It has been usual to collect fossil plants by splitting the pieces of rocks with a hammer. The more a stone has been exposed to the action of the weather, the easier it is to break it and lay bare what is within. But fossil plants found under such circumstances are no longer in good preservation: they have suffered greatly from exposure to the weather, and generally only the outlines are visible; their structure and the finest veins of the leaf-skeleton are lost. Stone when it has not been exposed to the air is not easily split; the more compact it is the more difficult it will be found to obtain the fossils in this way. Under favourable circumstances only fragments of the fossils are obtained. By the forcible splitting of pieces of rocks with a hammer it is only possible to succeed very imperfectly in obtaining fossil plants, besides which it must always be a lucky chance that the hammer strikes that part of the stone in which the plants lie concealed, and that it has not been injured by the blow, for a large number of fossils are lost in this way, or remain undiscovered in the stone. I have found a method by which fossil plants can be satisfactorily got out of the most compact rocks without using a hammer.

The pieces of rocks are for a considerable time subjected to a thorough soaking under the pressure of two or three atmospheres. In an iron vessel full of water brought into connexion with a stand-pipe the stones are left lying for half a year (most advantageously in summer-time). In those places where there is a fossil in the stone the material of the stone is not continuous. Thus numerous, often microscopically small, splits and other hollow spaces are found along the fossil plants. These hollow spaces get filled little by little with water. Then the stones which have been treated in this manner are exposed to an intense cold, −15° to 20° C. The water in the hollow spaces is turned into ice, and by this means the stones are burst asunder on the spot where there are petrifactions. The stones open of themselves, and show what they contain. The more compact the stone the surer and more complete by this method is the successful acquisition of the fossil plants. They show the original state in which they were preserved. With very hard stones the soaking and the subsequent freezing must be frequently repeated. On the first action of the frost the splits and hollow spaces are widened by the formation of ice within them to the surface of the stone. These must be quite filled again with

water; the stone will thus be raised to a higher temperature and again exposed to the soaking process. The ice formation and the soaking being thus employed alternately, the widening of the splits increases, till at last the stone opens of itself exactly along the enclosed fossil, which then comes to the light of day uninjured and in the best state of preservation.

This method offers not only the advantage of securing for investigation the most complete and well preserved fossil plants, but it yields also a much larger amount of material than could be obtained by the old method of forcibly splitting with a hammer. In this way no fossil can be lost. All the fossil plants in the stones are uninjured. Luck and chance are excluded. To obtain an abundant supply of useful material for investigation is of the greatest importance for the study of Phyto-Palæontology and must lead to better and surer results.

II.—*Method of Investigating Fossil Plants.*

Phyto-Palæontologists have hitherto made too many species. Unfortunately authors have been too readily disposed to adopt as a new species every slightly differing form. Consequently not only is science encumbered by a useless burden, but it is itself brought into a discredit which has occasioned serious injury to the progress of this branch of science. The most important way of remedying this evil, lies in procuring abundant material for investigation, showing a series of forms, and thus causing the false species to disappear. A collection of fossil plants acquired by careful study must therefore contain not only rare specimens, but as large a number as possible of a series of forms of common fossils. These series should be divided into two groups, the series of the contemporaneous, and of the non-contemporaneous (genetic) forms. The first is obtained by the bringing together the forms of a fossil out of the extension of one and the same layer (horizontal extension), the second in the searching for a fossil in different horizons (vertical extension). The latter series supplies the material for the phylogeny of the species, the complete elucidation of which is of the highest importance for the history of the development of the vegetable world.

A second way of removing the above-mentioned unsatisfactory state of things would be to put aside certain obsolete notions and prejudices. People are prone to admit mere differences of stratigraphical position as sufficient ground for the acceptance of a particular species, when indeed there appears to be no substantial reason arising out of its distinctive character. Only too often an insignificant difference of form, then regarded as important, is held to justify the acceptance of a species, if the fossil belongs to another horizon or another formation. My experience, however, has led me to the conclusion, that, in many cases, one species passes through many horizons and indeed through greater periods,

and that the number of the species is reduced all the more rapidly the more remote the Flora is from that of the present world. But of this more later on.

The method of investigating fossil plants must, above all things, be directed to their exact classification, and consequently to a knowledge of the facts on which the history of the development of the vegetable kingdom is supported. This however is only made possible by most carefully comparing fossil plants with living ones. Unfortunately, in this respect, so many faults and mistakes have been committed, that the greater part of the determinations as yet arrived at require revision and correction. Hitherto the fossils have not been compared accurately enough with the recent vegetable world. It may be frankly said, that most phyto-palæontologists possess too little botanical knowledge; how can it be expected of a novice in botany, that he should classify fossil plants correctly, if he do not thoroughly know the living ones?

The most frequent difficulties arise in classifying the fossil leaves which form by far the greatest number of fossil plant remains. The leaf skeleton which offers the most important marks for their classification must first be studied with this object, for the systematic botanists have barely regarded this matter at all. I may indeed point out, as a very fortunate circumstance, that exactly at the time I was much occupied with this work, Nature Printing was invented in the State Printing Office, at Vienna (1852), an operation by which the leaves of living plants with all the details of their finest veins were printed off in the most accurate manner.

I was permitted to publish a series of works on the leaf-skeleton together with illustrations in nature printing with the object of comparing them with fossil plants. The marks on the leaf-skeleton were examined and arranged, and at present all the families of living plants which are of importance in relation to the fossil Flora have been already brought into scientific order according to their leaf-skeletons.

III.—*Object and Plan of the Investigation of Fossil Plants.*

Fossil plants are often examined only for palæontological or geological purposes, but in the opinion of the author it is also necessary to consider the interests of botany. We must in this always take our departure from the known to discover the unknown. We proceed, therefore, from the Flora of the present world, step by step, to the primæval, and thus have first to investigate the Cainozoic Flora. Only when these have been fully examined and their connexion with the living Flora completely ascertained, can the Mezozoic Flora be so worked out that the genetic connexion of the Cainozoic Flora with the latter will be determined. The final object of these labours will be the investigation of the Palæozoic Flora,

and through them the question of the origin of the vegetable kingdom will receive such an answer as is open to human inquiry.

How is it possible to discover the genetic connexion of Floras following each other in immediate succession?

The successive Floras of different ages are not sharply distinguished from each other, but there are the most manifold transitions between them. These transitions are to be found in the common species. It is therefore desirable closely to examine, in the above-mentioned method, the species most frequently met with, and specially to select from the different varieties the progressive and retrogressive forms. By placing together these with other varieties discovered, in a vertical direction (that is, crossing the horizons lying over each other), the Phylogenetic series are obtained, and therewith also the required connecting links of the Floras.

As examples of the Phylogenetic series, only those of the *Castanea atavia* and of the *Pinus palæo-strobus**), found by me, are at present known. Other Phylogenetic series which I have discovered will be published at a future time.

IV.—*Results relating generally to the Tertiary Flora.*

My method of procuring fossil plants, and the improved method of investigation on the one part, and on the other the direction of the inquiry which I adopted, have led me to results which are very little in harmony with those obtained by the old method. I can only describe most of the previously determined species as being some of them incorrect, and the others of no value, inasmuch as the knowledge respecting them has been derived from insufficient materials. I shall probably, however, not be in a position to adduce special proof of this, and so correctly to determine which the false species are. On account of much new work, I must be satisfied to refer to it generally, and leave it to future specialists to relieve science from the mistakes which have been made.

I have found:—

Firstly, that all the Floras of the earth stand in genetic connexion with the Tertiary Flora. These contain the original species of the recent Flora and plant forms of all parts of the globe. The mixing together of forms of plants is clearly shown, especially in the Miocene Flora, as I at first pointed out in the Tertiary Flora of Austria.

Secondly, that in each of the recent Floras are to be perceived the elements of their common original Flora. They have, however, been more or less changed, and appear frequently altered into manifold forms. I have given the name of "Florenglieder" (members of a Flora) to these extensively-developed Flora elements. The character of a Flora

* "Beiträge zur Erforschung der Phylogenie der Pflanzenarten," "Denkschriften der Wiener Akademie der Wissenschaften," Band xxxviii.

has formed itself through the greater development of one element which has become the "Haupt-Florenglied" (principal member of a Flora); such as, for instance, has occurred in the Flora of Australia,* and of the Cape.† The rest of the genetic members have remained rudimentary. The Endemic species of European, Asiatic, and East Indian genera are, in the above-mentioned Floras, the representatives of these "Nebenglieder" (secondary members).

Thirdly, that the species of fossil plants inclined much more to the formation of varieties than those of living plants, and that the varieties of the fossil species, for the most part, correspond with the species of existing Flora. I have proved this in the case the *Pinus palæo-strobus*, the varieties of which so entirely correspond with many of the recent Pinus species, that the former must be recognised as the original forms of the latter. At some future time, I hope to publish a demonstration of the genetic connexion of the varieties of many other Tertiary plants with species of plants in the living world.

V.—*Results relating to the Eocene Flora of Great Britain.*

The very extensive materials which I have had under examination were principally those of the collections of the British Museum and that of Mr. John S. Gardner, and I have here to express to Mr. H. Woodward and Mr. Carruthers, as well as to Mr. Gardner, my most grateful thanks for their willing aid. I desire, also, especially to acknowledge my deep obligation to the Royal Society, from which I have received a grant for the investigation of the Eocene Flora of Great Britain. Mr. Gardner has gained for himself well deserved acknowledgments for the important services he has rendered in discovering and obtaining a vast collection of the Eocene Flora of Great Britain, and it has given me great satisfaction to have been associated with him in the study of this fossil Flora.

As the geology of the localities of the Eocene Flora of Great Britain has been already published by Mr. Gardner, I proceed at once to those results which the investigation of this Flora have, up to the present time, produced. These results can only be partially indicated now, as the comparing of the fossil Flora of Great Britain with other Floras will not be published until the investigations are completed. For the present, the monographic work of the *Filices* is finished in manuscript.

The Eocene Flora of Great Britain is distinguished by a series of tropical forms of ferns. Of these are especially to be named the peculiar genera of *Podoloma* and *Glossochlamys*, which connect themselves mostly with tropical forms of *Polypodium;* then the peculiar genus *Menyphyllum* most nearly related to the tropical *Aspidiaceæ.*

* Ettingshausen, "Die genetische Gliederung der Flora Australiens," "Denkschriften der Wiener Akademie der Wissenschaften," Band xxxvii.

† Ettingshausen, "Die genetische Gliederung der Cap-Flora," "Sitzungsber. der Wiener Akademie der Wissenschaften," Band lxxi.

In addition may be mentioned forms of *Chrysodium* and *Lygodium.* The appearance of the genus *Gleichenia* reminds us of the fern Flora of the Cretaceous period, while some species of *Pteris* and *Phegopteris* are related to species of the Miocene Flora. One fern, *Asplenites allusoroides* Ung, as yet only known in the Fossil Flora of Sotzka, has also here found its predecessor.

The species of the Eocene Flora of Great Britain are enumerated as follows:—

Filices of the Eocene Flora of Great Britain.

Names of Species.	Localities.	Formation.
ORD. POLYPODIACEÆ.		
a. Acrostichaceæ.		
Chrysodium Lanzæanum. *Vis sp.* ..	Studland, Bournemouth.	Lower and Middle Eocene.
b. Polypodieæ.		
Podoloma polypodioides. *Ett. et Gard.* ..	Bournemouth ..	Middle Eocene.
„ affine. *Ett. et Gard.* ..	„ ..	„ „
Glossochlamys transmutans. *Ett. et Gard.*	„ ..	„ „
Polypodium sp., near to P.	„ ..	„ „
„ lepidotum. *Willd.* ..	„ ..	„ „
c. Pterideæ.		
Adiantum Carruthersii. *Ett. et Gard.* ..	„ ..	„ „
Pteris eocenica. *Ett. et Gard.*	„ ..	„ „
„ Bournemouthiana. *Ett. et Gard.* ..	„ ..	„ „
„ pseudo-pennæformis. *Lesq.* ..	Counter Hill ..	Lower Eocene.
d. Aspleniaceæ.		
Asplenites præ-allusoroides. *Ett. et Gard.*	Bournemouth ..	Middle Eocene.
e. Aspidiaceæ.		
Menyphyllum elegans. *Ett. et Gard.* ..	„ ..	„ „
Phegopteris præ-cuspidata. *Ett. et Gard*	„ ..	„ „
Phegopteris Bunburii. *Heer*	Bovey Tracey ..	„ „
	Bournemouth ..	„ „
ORD. GLEICHENIACEÆ.		
Gleichenia hantonensis. *Wanklyn sp.* ..	Bournemouth ..	„ „
ORD. SCHIZÆACEÆ.		
Lygodium Kaulfussi. *Heer*	„ ..	„ „
ORD. OSMUNDACEÆ.		
Osmunda subcretacea. *Saporta* ..	„ ..	„ „
„ lignitum. *Gieb. sp.*	Bovey Tracey ..	„ „
	Bournemouth ..	„ „

The Society then adjourned over the Christmas Recess to Thursday, January 9, 1879.

Presents, December 5, 1878.

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January 9, 1879.

W. SPOTTISWOODE, M.A., D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Researches on the Absorption of the Ultra-Violet Rays of the Spectrum by Organic Substances." By W. N. HARTLEY, F.Inst. Chem., F.R.S.E., F.C.S., Demonstrator of Chemistry, King's College, London, and A. K. HUNTINGTON, F.Inst. Chem., A.R.Sc. Mines, F.C.S. Communicated by Professor G. G. STOKES, Sec.R.S. Received October 10, 1878.

(Abstract.)

Parts I and II.

One of the authors of this paper, Mr. Hartley, having studied the researches of the late Dr. W. A. Miller "On the Photographic Transparency of Various Bodies," &c. ("Phil. Trans.," 1863, I), and of Professor Stokes, "On the Long Spectrum of Electric Light" ("Phil. Trans.," 1863, I), determined to study the action of organic substances on the ultra-violet spectrum. In 1872, the apparatus of Dr. Miller was reconstructed, and some experiments were made which showed that it was capable of some slight improvements. Some time was spent in testing the value of the photographic method of experimenting as compared with that adopted by Professor Stokes, and preference was eventually given to the former, or rather to a combination of both methods, since occasional use was made of a focussing screen either of uranium glass or of white paper steeped in æsculine solution and dried. It was soon apparent that a wide field of investigation was opened, and, with the assistance of Mr. Huntington, a systematic course of examination of organic compounds was commenced at the beginning of the present year. In January, 1878, M. Soret published his "Recherches sur l'Absorption des Rayons Ultra-Violet par diverses substances" ("Archives des Sciences Physiques et Naturelles, Genève"), which includes the examination of many inorganic and some organic compounds. Though this is a work of very great interest, it does not touch upon the subject of the present investigation, namely, the connexion between chemical constitution and diactinic quality. M. Soret uses a spectro-

scope of his invention which receives the ultra-violet rays upon a fluorescent eye-piece, and so renders them visible.

The Apparatus.—This consisted of a spectroscope attached to a photographic camera, the prism and lenses being of quartz. The electric light consisted of sparks of great intensity passed between metallic electrodes. To produce the sparks an induction coil, capable of giving a 7-inch spark in air, was excited by five cells of Groves' battery. A Leyden jar was interposed between the coil and the electrodes, each surface of the foil measuring 72 square inches. The electrodes found to answer best were points of nickel wire, containing a trace of copper. Cells of glass, with quartz sides, were used for holding liquids under examination. These cells were placed behind the slit of the spectroscope, the spark passing in front; volatile liquids were thus prevented from taking fire, and a certain loss of light was avoided. No condensing lens was used in front of the slit, because occasionally it was found convenient to employ an amalgam, containing zinc, cadmium, aluminium, and magnesium, dissolved in mercury, in conjunction with a point of iron, and under these circumstances volatilised mercury would condense on the lens.

The Photographic Process.—It was found by experiment that a wet collodion process, as used by Dr. Miller, was disadvantageous for several reasons, and therefore dry plates were used. A preference was given to gelatine pellicle plates, containing bromide of silver. They are quite sufficiently sensitive, give a very finely defined picture, and do not necessitate a varnishing process. The exposure has generally been about 10 seconds, but on certain occasions plates have been in the camera for an hour and a half. We have found no difficulty in obtaining a constant stream of sparks, giving a steady light for three-quarters of an hour without intermission.

The Measurement of Absorption-Bands, &c.

In order to measure the degree of absorption exercised by different substances the example of M. Soret has been followed, and the lines of cadmium have been taken for the purpose. M. Mascart has measured the wave-length of these lines both for the visible and the ultra-violet rays. Sometimes measurements on the scale of wave-length have been adopted, but in other cases it has been found more convenient to make use of spectra as photographed. Photographs of different metallic spectra employed are presented. The lines of cadmium are distinguished by the numbers assigned to them by M. Mascart. A comparison is also given of the relative extent of the visible and ultra-violet rays after passage through a prism.

The prism was placed at the angle of minimum deviation for the sodium line D.

The various parts of the apparatus are screwed down so as to be immovable after a proper adjustment.

The Examination of Organic Substances.

Dr. Miller failed to trace any connexion between the chemical complexity of a substance and its actinic absorption.

With the view of ascertaining whether any such connexion existed an examination was made of the normal alcohols, the normal fatty acids, and a series of ethereal salts. Great trouble was occasioned by the interference of minute traces of otherwise undetected impurities, the presence of which was often unaccountable. Four diagrams, showing the relative transparency of different substances, illustrate this part of the paper, and from the results obtained the following conclusions have been drawn.

(1.) The normal alcohols of the series $C_nH_{2n+1}OH$ are remarkable for transparency to the ultra-violet rays of the spectrum, pure methylic alcohol being as nearly so as water.

(2.) The normal fatty acids exhibit a greater absorption of the more refrangible rays of the ultra-violet spectrum than the normal alcohols containing the same number of carbon-atoms.

(3.) There is an increased absorption of the more refrangible rays corresponding to each increment of CH_2 in the molecule of the alcohols and acids.

(4.) Like the alcohols and acids, the ethereal salts derived from them are highly transparent to the ultra-violet rays, and do not exhibit absorption-bands.

In order to ascertain whether isomeric bodies exhibited similar or identical absorption spectra a series of benzene derivatives was examined. From the great absorptive power of this class of substances it was found necessary to use very dilute solutions even though the cells holding the liquids were not more than 0·75 inch in thickness. Curves were plotted by taking the proportions of substances in solution as ordinates, and the position of absorption-bands as abscissæ, and these curves are highly characteristic features of very many compounds. About twenty diagrams have thus been made.

The following is a summary of the chief points of interest appertaining to benzene and its derivatives.

(1.) Benzene, and the hydrocarbons, the phenols, acids, and amines derived therefrom, are remarkable firstly, for their powerful absorption of the ultra-violet rays; secondly, for the absorption-bands made visible by dissolving them in water or alcohol, and diluting; and thirdly, for the extraordinary intensity of these absorption-bands, that is to say, their power of resisting dilution.

(2.) Isomeric bodies, containing the benzene nucleus, exhibit widely

different spectra, inasmuch as their absorption-bands vary in position and in intensity.

(3.) The photographic absorption spectra can be employed as a means of identifying organic substances, and as a most delicate test of their purity. The curves obtained by co-ordinating the extent of dilution with the position of the rays of the spectrum absorbed by the solution form a strongly marked and often a highly characteristic feature of many organic compounds.

There is a curious feature in connexion with the position of the absorption bands; at the less refrangible end they either begin at line 12 Cd or line 17 Cd, and those which begin at 12 end a little beyond 17.

No naphthalene or anthracene derivatives have yet been examined, and very few substances of unknown constitution—hence most interesting results may be anticipated from a continuation of this research, and this contribution must be accepted rather as a bare commencement of the subject than its conclusion.

II. "On the Electromagnetic Theory of the Reflection and Refraction of Light." By GEORGE FRANCIS FITZGERALD, M.A., Fellow of Trinity College, Dublin. Communicated by G. J. STONEY, M.A., F.R.S., Secretary of the Queen's University, Ireland. Received October 26, 1878.

(Abstract.)

The media, at whose surfaces reflection and refraction are supposed to take place, are assumed to be non-conductors, and isotropic as regards magnetic inductive capacity. Some reasons are advanced why the results should apply at least approximately to conductors. In the first part of the paper the media are not assumed to be isotropic as regards electrostatic inductive capacity, so that the results are generally applicable to reflection and refraction at the surfaces of crystals. I use the expressions given by Professor J. Clerk Maxwell in his "Electricity and Magnetism," vol. ii, Part IV, chap. 11, for the electrostatic and electrokinetic energy of such media. By assuming three quantities, ξ, η, and ζ, such that, t representing time, $\frac{d\xi}{dt}$, $\frac{d\eta}{dt}$, and $\frac{d\zeta}{dt}$, are the components of the magnetic force at any point, I have thrown these expressions for the electrostatic and electrokinetic energy of a medium into the same forms as M'Cullagh assumed to represent the potential and kinetic energy of the ether, in "An Essay towards a Dynamical Theory of Crystalline Reflection and Refraction," pub-

lished in vol. xxi of the "Transactions of the Royal Irish Academy." Following a slightly different line from his, I obtain, by a quaternion and accompanying Cartesian analysis, the same results as to wave propagation, reflection, and refraction, as those obtained by M'Cullagh, and which he developed into the beautiful theorem of the polar plane. Of course, the resulting laws of wave propagation agree with those obtained by Professor Maxwell from the same equations by a somewhat different method. For isotropic media, the ordinary laws of reflection and refraction are obtained, and the well-known expressions for the amplitudes of the reflected and refracted rays.

In the second part of the paper I consider the case of reflection at the surface of a magnetised medium, adopting the expressions Professor J. Clerk Maxwell has assumed in "Electricity and Magnetism," vol. ii, Part IV, § 824, to express the kinetic energy of such a medium. From this, following the same line as before, I have deduced the following equations to represent the superficial conditions: In them, ξ, η, ζ, have the same meaning as before, and the axes are x in the intersections of the plane of incidence and the surface, y in the surface, and z normal to it; α, β, γ, are the components of the strength of the vortex that Professor Maxwell assumes, and $\frac{d}{dh}=\alpha\frac{d}{dx}+\beta\frac{d}{dy}+\gamma\frac{d}{dz}$, which, with these axes, reduces to $\alpha\frac{d}{dx}+\gamma\frac{d}{dz}$; K and K_1 are the electrostatic inductive capacities of the two media in contact, and the quantities referring to one of these which is supposed to be non-magnetic are distinguished by the suffix $_1$; C is a constant, on which the power of the medium to rotate the plane of polarisation of light depends.

$$\frac{d\xi_1}{dz_1}-\frac{d\zeta_1}{dx_1}=\frac{K_1}{K}\left(\frac{d\xi}{dz}-\frac{d\zeta}{dx}\right)-4\pi CK_1\left\{\gamma\frac{d^2\eta}{dzdt}+\frac{d^2\eta}{dhdt}\right\}$$

$$\frac{d\eta_1}{dz_1}=\frac{K_1}{K}\cdot\frac{d\eta}{dz}+4\pi CK_1\left\{\gamma\frac{d}{dt}\left(\frac{d\xi}{dz}-\frac{d\zeta}{dx}\right)+\frac{d^2\xi}{dhdt}\right\}.$$

As these are unchanged by a simultaneous alteration of the signs of η and C, I show that the method adopted in my former paper on Magnetic Reflection in the "Proceedings of the Royal Society," for 1876, No. 176, is justified, and that it is legitimate to consider an incident plane polarised ray as composed of two oppositely circularly polarised rays, each of which is reflected according to its own laws. From these I further deduce that, when the magnetisation of the medium is all in the direction of η, there is no effect on reflection or refraction produced by it. I consider next the cases of the magnetisation being all normal to the surface, and all in the surface and the plane of incidence, and obtain the following result: When the incident ray is plane polarised, and the plane of polarisation is either in or

perpendicular to the plane of incidence, the effect of magnetisation is to introduce a component into the reflected ray perpendicular to the original plane of polarisation, whose amplitude, c, is given in the several cases by the following equations, in which i is the angle of incidence, and r of reflection, and k a small constant depending principally on C, and the intensity of the incident ray:—1. When the magnetisation is normal to the reflecting surface. If the incident ray be polarised in the plane of incidence—

$$c=k\,.\,\frac{(1+\cos^2 r)\sin^2 i\sin 2i}{\sin r\,.\,\sin^2(i+r)\,.\,\cos(i-r)}.$$

If it be polarised in a plane perpendicular to the plane of incidence—

$$c=k\frac{\cos^2 r\,.\,\sin^2 i\sin 2i}{\sin r\,.\,\sin^2(i+r)\,.\,\cos(i-r)}.$$

2. When the magnetisation is parallel to the intersection of the surface and the plane of incidence, and the plane of polarisation of the incident ray is either in or perpendicular to the plane of incidence—

$$c=k\frac{\cos r\sin^2 i\sin 2i}{\sin^2(i+r)\cos(i-r)}.$$

This vanishes at the grazing and normal incidences, and, in the case of iron, attains a maximum at about the angle of incidence $i=63^\circ\ 20'$.

I do not obtain any change of phase by reflection in any case; and this is to be expected, as this change of phase probably depends on the nature of the change from one medium to another, which, following M'Cullagh, I have uniformly assumed to be abrupt. Apart from this question of change of phase, my results conform completely to Mr. Kerr's beautiful experiments on the reflection of light from the pole of a magnet, as published in the Philosophical Magazines for May, 1877, and March, 1878.

III. "On Dry Fog." By E. Frankland, D.C.L., F.R.S., Professor of Chemistry in the Royal School of Mines. Received October 29, 1878.

It has often been noticed, especially in and near large towns, that a foggy atmosphere is not always saturated with moisture: thus on the 17th of October last, at 3·30 p.m., during a thick fog in London, the degree of humidity was only 80 per cent. of saturation; and Mr. Glaisher, in his memorable balloon ascents, observed that in passing through cloud or fog the hygrometer sometimes showed the air to possess considerable dryness. In the ascent from Wolver-

hampton, on July 17, 1862, at an altitude of 9,882 feet, and when passing through a cloud so dense that the balloon could not be seen from the car, the dry bulb thermometer read 37°·8 F. and the wet 30°·2, indicating a dew point 17°·9 below the air temperature. Again, on the 30th July in the same year, when at an altitude of 6,466 feet, between the Crystal Palace and Gravesend, and whilst the balloon was passing through a "great mist," the dew point was 12°·7 F. below the temperature of the air. The following is a tabulated statement of other instances in which there was comparative dryness of the air in the midst of cloud or fog:—

Date.	Place of ascent.	Altitude in feet.	Temperature of air.	Degree of humidity. 100 = saturation.
1862.				
August 18th	Wolverhampton	5,922	53°·5 F.	61
1863.				
March 31st	Crystal Palace	3,698	38·5 „	62
April 18th	„ „	9,000	32·5 „	52
„ „	„ „	8,000	34·9 „	73
„ „	„ „	7,000	37·8 „	87
„ „	„ „	6,000	41·0 „	76
„ „	„ „	5,000	45·0 „	67
June 26th	Wolverton	11,000	30·0 „	68
„ „	„	10,000	31·5 „	46
July 11th	Crystal Palace	3,200	65·2 „	57
„ „	„ „	2,600	.. ..	53
„ „	„ „	1,600	.. ..	50
„ „	„ „	1,000	64·7 „	53
1864.				
April 6th	Woolwich	6,000	44·0 „	64
„ „	„	1,000	41·7 „	75
1865.				
Feb. 27th	„	4,400	42·0 „	52

It is thus evident that the air closely surrounding the spherules of water in a mist, cloud or fog, is sometimes far from saturated with moisture; although, as is well known to persons occupied with gas analysis, when a perfectly dry gas is admitted into a moist eudiometer it very rapidly assumes the volume indicating saturation, notwithstanding that the proportion of water surface to volume of gas is obviously far less than that afforded to the interstitial air of a fog.

In a special experiment of this kind, it was found that air dried over calcic chloride became completely saturated with moisture in less than one minute and fifty seconds, when it was passed into a moist glass tube three-fourths of an inch in diameter. It appeared to me, therefore, interesting to inquire under what condition the evaporation from the surface of water can be so hindered as to cause this delay in the saturation of the closely surrounding air. Many years ago I became acquainted with the fact, first noticed I believe by Mr. P.

Spence of Manchester, that the evaporation of saline solutions, kept just below their boiling point in open pans, can be almost entirely prevented by covering the liquid with a thin stratum of coal-tar. It was thus that Mr. Spence effected a considerable saving of fuel in that part of the process of manufacturing alum in which burnt aluminous shale is digested for many hours with hot dilute sulphuric acid; much less fuel being of course required when the digestion was carried on without evaporation, than when steam escaped from the surface of the hot liquid. This simple though important technical application suggested to me a condition of things under which the existence of so-called "dry fog" would be possible. From our manufactories and domestic fires, vast aggregate quantities of coal-tar and paraffin oil are daily distilled into the atmosphere, and condensing upon, or attaching themselves to, the watery spherules of fog or cloud, must of necessity coat these latter with an oily film, which would, in all probability, retard the evaporation of the water, and the consequent saturation of the interstitial air.

The following experiments were made in order to test the validity of this explanation :—

I. Two platinum dishes, containing water and presenting equal surfaces of liquid, were placed side by side in a moderate draught of air; the water in one being coated by a very thin film of coal-tar. By comparing the loss of weight in the two dishes, it was found that during twenty-four hours the evaporation was reduced by the film of coal-tar from 7·195 grms. to 1·124 grms. or 84·4 per cent.

II. In a similar experiment, the evaporation during twenty-four hours was reduced from 7·986 grms. to 1·709 grms., or 78·6 per cent.

In order to imitate more nearly the *modus operandi* of actual smoke in foggy air, the smoke from burning coal was in the next experiments blown upon the surface of the water in one of the platinum dishes, the dishes being placed as before in a draught niche.

III. The evaporation during eighteen hours was reduced from 4·26 grms. to ·969 grm., or 77·3 per cent.

IV. In another experiment, the evaporation during twenty-four hours was diminished from 6·325 grms. to 1·173 grms., or 81·5 per cent., and during forty-two hours from 10·585 grms. to 2·142 grms., or 79·8 per cent.

So far the experiments were made in a current of ordinary air of varying humidity; but they were afterwards repeated with the following results, under a large bell-jar, in which the enclosed air was continually dried by a large surface of concentrated sulphuric acid. As in the last two trials, the film was produced by coal smoke.

V. During forty-eight hours, evaporation was diminished from 5·178 grms. to ·737 grm., or 85·8 per cent.

VI. During twenty-two hours, evaporation was reduced from 2·123 grms. to ·668 grm., or 68·5 per cent.

VII. During twenty-four hours, the reduction was from 2·460 grms. to ·180 grm., or 92·7 per cent.

VIII. In a period of seventy-two hours, the reduction was from 7·638 grms. to ·917 grm., or 88 per cent.

IX. In seventy hours, the evaporation was diminished from 7·732 grms. to 2·586 grms., or 66·6 per cent.

X. In forty-six hours, the diminution was from 4·973 grms. to 1·647 grms., or 66·9 per cent.

Experiments were also made with single drops of water suspended in loops of fine platinum wire, and placed in the bell-jar filled with dry air; but it was found that the oily film had a strong tendency to leave the drop and run up the platinum wire. In a comparative experiment, in which one of the drops was protected by a coal-smoke film, the unprotected drop lost 90 per cent. of its weight in two and a half hours at 16°·6 C.; whilst the protected drop lost only 37·8 per cent. at 17°·8 C. in the same time. Another drop, protected by a film of coal-tar, lost 37·6 per cent. of its weight in two and a half hours, the temperature being 14° C. in the bell-jar.

It is highly probable that if globules of water without any solid support (like those in cloud and fog) could have been operated upon, the retardation of evaporation would have been still more marked, or perhaps altogether arrested; for in all the above experiments the oily films manifested a tendency to break up and attach themselves to the solid support of the water, leaving the surface of the latter partially unprotected.

The results of these experiments point out a condition of very common occurrence, competent to produce "dry fog," whilst they also explain the frequency, persistency, and irritating character of those fogs which afflict our large towns; inasmuch as some of the products of destructive distillation of coal are very irritating to the respiratory organs, and a large proportion of them is scarcely if at all volatile at ordinary temperatures.

My thanks are due to my pupil, Mr. C. G. Matthews, for his assistance in the foregoing quantitative determinations.

IV. "Note on the Inequalities of the Diurnal Range of the Declination Magnet as recorded at the Kew Observatory." By BALFOUR STEWART, F.R.S., Professor of Natural Philosophy in Owens College, Manchester, and WILLIAM DODGSON, Esq. Received November 18, 1878.

We are at present engaged in searching for the natural inequalities

of the above range, more especially for any of which the period is between 24 and 25 days. We find strong evidence of an inequality of considerable magnitude of which the period is 24·00 days, very nearly. We have also found preliminary evidence of the existence of two considerable inequalites of periods not very far from 24·65 and 24·80 days. These two appear to come together in about 11 years, but we cannot yet give the exact time of this.

We have not found a trace of any inequality with a period of 24·25 days.

V. "Some Experiments on Metallic Reflexion." By Sir JOHN CONROY, Bart., M.A. Communicated by Professor G. G. STOKES, Sec. R.S. Received November 18, 1878.

In the experiments made by Sir David Brewster, M. Jamin, Professor Haughton, and others, on the light reflected by polished metallic surfaces, the reflecting surfaces were in contact with air; and, as far as I am aware, the only observations which have been made when the reflecting surfaces were in contact with other media are those by Quincke, an account of which is given in "Poggendorff's Annalen," vol. cxxviii, p. 541, and in the "Jubelband," p. 336. He found that he obtained different values for the principal incidence and principal azimuth, according as the reflecting surface of a film of silver was in contact with air, crown glass, flint glass, water, or turpentine, and that the only connexion between the values of these angles and the refractive index of the medium in which the reflexion took place was, that in general with the same metal, the principal incidence and the principal azimuth became less as the refractive index of the medium increased.

I therefore hope that a short account of some attempts that I have recently made to determine the principal incidence for, and the principal azimuth of, the light reflected by polished surfaces of gold and copper in contact with different media, may be of interest.

The experiments are, I regret to say, incomplete, as, finding that my eyes were beginning to suffer, I thought it best, for the present at least, to discontinue them.

I used a Babinet's goniometer, to the arms of which two tubes containing nicols were attached, a vertical divided circle being fixed at one end of each, so that the position of the nicols could be read by a vernier to 5′. The goniometer had, in addition to the horizontal stage, a vertical one, so arranged that the reflecting surface could be placed in the axis of the instrument; toothed wheels, working into a pinion rotating on an axis fixed in one of the arms of the divided circle, were attached to the vertical stage, the position of which could

be read by a vernier to 15″, and to the telescope arm; the ratio of the wheels to the pinion being such that, on moving the telescope arm, the vertical stage also moved in the same direction, but with half the angular velocity; so that when the reflecting surface had been properly adjusted, the light which passed along the axis of the tube fixed to the collimator arm, was reflected along the axis of that fixed to the telescope arm, at all angles of incidence.

A quarter undulation plate was placed at the inner end of the tube fixed to the collimator arm, and a small direct vision spectroscope, with a photographic scale, could be attached to the other tube. The lower edge of the vertical stage being some distance above the graduated circle of the goniometer, a cylindrical vessel of thin glass, about 6 centims. in diameter, could be placed on the horizontal stage, so as to surround the lower part of the reflecting surface; this being filled with the liquid, and a narrow vertical slit placed so as to limit the incident light, fairly good observations could be made when the reflecting surface was in contact with various liquids.

When the principal section of the first or polarizing nicol was inclined at an angle of 45° to the plane of incidence, and one of the neutral axes of the quarter undulation plate placed in that plane, the transmitted light was elliptically polarized; and at a particular incidence, varying with its refrangibility, it was reflected by the metallic surface as plane polarized light; the plane of polarization being determined by the second nicol.

Had the retarding plate really been "a quarter undulation plate" for light of any given wave-length, the angle of incidence at which it was reflected as plane polarized light, and the azimuth of its plane of polarization, would have been the principal incidence and principal azimuth for light of that refrangibility.

The retardation of a given plate varies so much for different portions of the spectrum, that even had it been possible to obtain one producing a retardation of exactly 90° for light of any definite refrangibility, it would have differed greatly from a quarter plate for other portions of the spectrum.

Both the neutral axes of the plate were successively placed in the plane of incidence, and the mean of the two values of the angle of incidence taken as the principal incidence.

This arrangement is very similar to the one used by Dr. Eilhard Wiedemann in his observations on the light reflected by surfaces of fuchsine and copper, and described in "Pogg. Ann.," vol. cli, p. 6. In Dr. Wiedemann's experiments the angle of incidence remained constant, the position of the quarter undulation plate and of the nicol being varied; whilst in mine, the position of the quarter undulation plate was constant, and the angle of incidence and the position of the nicol were altered. By this means the principal incidence and azimuth

were determined directly, but less accurately than by Dr. Wiedemann's arrangement, which, however, involves a good deal of calculation.

The analysing portion of Dr. Wiedemann's instrument appears to differ merely by the addition of a small direct vision spectroscope from the Elliptic Analyser of Professor Stokes, described in the "Report of the British Association for 1851," Part II, p. 14.

The experiments were made with sun-light and with lamp-light; with the former, the angle of incidence and the azimuth of the analysing nicol were altered till the dark band in the spectrum was most intense at certain definite positions, as measured by the scale of the spectroscope; with the latter, till the light which had passed through a piece of red glass was reduced to a minimum.

Numerous measurements were made of both these angles with a plate of gold in air, water, carbon bisulphide, and carbon tetrachloride; and of copper, in air, water, and carbon tetrachloride; but the results were not very satisfactory. In addition to the difficulty of determining accurately the zero of the nicols, and of placing the neutral axis of the quarter undulation plate in the plane of incidence, I found that very different values were obtained for the principal incidence, according as one or other of the neutral axes of the quarter undulation plate I was using was in the plane of incidence.

In all cases, however, the principal incidence which, as is well known, is less for the more refrangible rays, diminishes, and the principal azimuth increases with the increase of the refractive index of the medium in contact with the metallic surface; and further, the diminution in the value of the principal incidence appears to be nearly in proportion to the increase of the refractive index of the surrounding medium.

The decrease of the principal incidence, with an increase in the refractive index of the surrounding medium, is exactly what might be expected to take place if the principal incidence for a metal were the same as the angle of polarization of a transparent substance; that is, the angle whose tangent is equal to the refractive index.

If such is the case, the metals must all have very high refractive indices; but some experiments of Quincke's ("Pogg. Ann.," vol. cxix, p. 379, and vol. cxx, p. 602) appear to show that their refractive indices are less than 1.

The following are some results I obtained with a gold plate (formed by soldering a slip of thin sheet gold to a brass plate), in air, with lamp-light, a deep red glass being interposed; the position of the quarter undulation plate in which the ray polarized perpendicularly to the plane of incidence was retarded relatively to the other, being called A, and that in which the retarded ray was the one polarized in the plane of incidence, B. The signs of the azimuth of the plane of

polarization of the reflected light show which ray is retarded by the plate; and, to confirm this, the light transmitted by the nicol and plate was examined with an Iceland spar, cut to show the rings.

The azimuths are reckoned as positive when measured from the plane of incidence in the direction in which the hands of a watch move, to a person supposed to be so placed as to receive the light, whether incident or reflected, into his eye.

Quarter Undulation Plate at A.

Plane of polarization of incident light.	Principal incidence.	Principal azimuth.
°	° ′	° ′
+45	80 10	+36 05
	80 21	36 35
	80 14	35 25
	80 23	36 05
−45	80 14	−36 50
	79 50	34 55
	80 05	35 40
	80 27	35 25
Mean	80 13	35 52

Quarter Undulation Plate at B.

Plane of polarization of incident light.	Principal incidence.	Principal azimuth.
°	° ′	° ′
+45	71 12	−34 40
	71 03	35 25
	70 51	34 25
	71 02	34 30
−45	71 0	+37 30
	70 22	36 35
	70 07	36 35
	70 22	37 55
Mean	70 45	35 57

Similar measurements were made when the gold plate was in water and carbon bisulphide. The values of the incidences differed greatly according as one or other of the neutral axes of the quarter undulation plate was in the plane of incidence, the measurements being about as concordant as those made with the gold plate in air; the means of these determinations were taken as nearest the truth.

Since the retardation of the ray polarized perpendicularly to the

plane of incidence probably varies more for each degree when the light is incident at an angle greater than that of the principal incidence, than when it falls on the surface at a less angle, the mean of these two sets of determinations can only be considered as an approximation to the truth, especially when, as in this instance, the difference between the two values is a considerable one.

Mean value, from eight observations, four with the P. S. of the polarising nicol inclined to the right and four with it to the left of the plane of incidence, of the principal incidence and principal azimuth, for red light, with Quarter Undulation Plate 1.

Gold in air	Plate at A..	80° 13′	....	35° 52′
	" B..	70 45	....	35 57
	Mean value.	75 29	....	35 54
Gold in water	Plate at A..	76 46	....	37 13
	" B..	66 46	....	35 50
	Mean value.	71 46	....	36 31
Gold in carbon bisulphide.	Plate at A..	76 10	....	37 48
	" B..	62 44	....	37 40
	Mean value.	69 27	....	37 44

The principal incidence and principal azimuth for gold in air, with red light, were determined with six other quarter undulation plates with the following results; the numbers for Plate 1 being the mean of eight observations, whilst those of the remainder are the mean of two only, made with the polarizing nicol on either side of the plane of incidence:—

		Plate 1.	Plate 2.	Plate 3.	Plate 4.
Principal incidence	Plate at A...	80° 13′	78° 45′	80° 01′	79° 57′
	" B...	70 45	70 12	69 41	70 46
	Mean value..	75 29	74 28	74 51	75 21
Principal azimuth	Plate at A...	35 52	36 30	36 12	36 22
	" B...	35 57	35 40	36 02	35 10
	Mean value..	35 54	36 05	36 07	35 46

		Plate 5.	Plate 6.	Plate 7.
Principal incidence	Plate at A....	79° 57′	77° 43′	77° 50′
	" B....	69 56	73 26	73 23
	Mean value...	74 56	75 34	75 36
Principal azimuth	Plate at A....	36 0	34 55	34 52
	" B....	35 22	35 10	34 42
	Mean value...	35 41	35 02	34 47

In order to ascertain the probable error of the mean principal incidence and azimuth as determined with Plate 1, the measurements were repeated with Plate 6; the difference between the two values of the principal incidence, according as one or other of the neutral axes of the plate was in the plane of incidence, being least, and therefore the retardation for red light differing least from 90° for Plates 6 and 7.

Quarter Undulation Plate at A.

Plane of polarization of incident light.	Principal incidence.	Principal azimuth.
+45°	78° 25′	+35° 0′
	77 58	35 35
	77 42	35 05
	78 26	35 40
−45	78 24	−36 45
	77 47	35 30
	77 47	36 15
	77 59	36 0
Mean	78 03	35 43

Quarter Undulation Plate at B.

+45	73 57	−34 40
	73 17	36 10
	73 52	35 40
	73 35	35 20
−45	74 30	+35 20
	73 05	36 05
	73 37	34 35
	74 07	35 0
Mean	73 45	35 21

Similar measurements, which were about as concordant, were made with the gold plate in water and carbon bisulphide. The numbers in the table being the means of eight observations, four with the principal section of the polarizing nicol inclined to the right, and four with it to the left of the plane of incidence.

Gold in air	Plate at A.	78° 03′	35° 43′
	„ B.	73 45	35 21
	Mean value.	75 54	35 32

Gold in water	Plate at A..	74° 46′	....	36° 0′
	„ B..	70 25	....	36 49
	Mean value.	72 35	....	36 24
Gold in carbon bisulphide	Plate at A..	71 37	....	36 59
	„ B..	68 26	....	36 43
	Mean value.	70 01	....	36 51

The mean values of the principal incidence and principal azimuth obtained with the two quarter undulation plates being different, it was assumed that the errors of the means are as the squares of the small errors of the plates, and that the errors of the incidences in either position of the plate, and therefore the algebraical differences or numerical sums of the errors in the two positions, that is, the differences of the apparent principal incidence in the two positions, as the first powers; and therefore that the errors of the means are as the squares of the difference of incidences in the two positions.

Gold in Air.

	Plate 1.		Plate 6.		Correction.
Principal incidence	75° 29′	..	75° 54′	..	+25′
„ azimuth	35 54	..	35 32	..	−22
Difference of principal incidence in two positions	9° 28′ or 568′,		4° 18′ or 258′.		

Thus the residual corrections to the results got with Plate 6 will be to the difference on the results got by Plate 1 and Plate 6, as 258^2 to 568^2-258^2, or as No. log $\bar{1}\cdot41491$ to 1; this gives $+6'$ and $-5'$ making the corrected principal incidence and principal azimuth 76° and 35° 27′.

In a similar manner the means of the results got with the gold plate in water and carbon bisulphide were corrected, the final results being with red light.

	Principal incidence.		Principal azimuth.
Gold in air	76° 0′	....	35° 27′
„ water	72 46	....	36 23
„ carbon bisulphide	70 03	....	36 48

In order to determine the principal incidence and azimuth for gold by an independent method, the one originally used by Sir David Brewster was adopted; the quarter undulation plate was removed, and a second gold plate attached to the vertical stage in such a manner that, whilst the plates remained parallel to each other, the distance between them could be altered. The plates were so adjusted that

when the light was incident upon the surface of the first at angle of about 70°, it was reflected once by either plate.

The incident light being polarized in a plane inclined at an angle of 45° to the plane of incidence, the position of the stage and of the analysing nicol were altered till the reflected light was reduced to a minimum.

Plane of polarization of incident light.	Principal incidence.	Azimuth.
+45°	75° 45′	−30° 20′
	75 52	31 30
	75 23	31 05
	76 02	31 55
−45	75 54	+29 30
	76 0	29 50
	75 57	29 05
	76 03	28 45
Mean	75 52	30 15

A rectangular glass trough was placed on the horizontal stage of the goniometer so as to surround the gold plates; the trough filled with water, and the principal incidence and the azimuth observed.

The ray of light which had been twice reflected by the plates being parallel to the incident ray, and the trough having been placed with its front perpendicular to the direction of the incident light, the polarization of the ray could not be altered in any way by the glass, as indeed was verified by experiment.

The light having been twice reflected, the square root of the tangent of the angle which the plane of polarization of the reflected ray makes with the plane of incidence, is equal to the tangent of the principal azimuth.

The principal incidence and principal azimuth determined by this method from eight observations, four with the plane of polarization of the incident light on either side of the plane of incidence are—

Gold in air	75° 52′	37° 22′
„ water	72 28	37 48

The principal incidences agree fairly well with those obtained by the other method; but the azimuths are somewhat higher.

The following table contains the values of these constants for gold in air, as previously determined by—

Sir David Brewster.	° ′		° ′	
("Optics," ed. 1853, p. 309, 311)..	70 45	..	33 0	for jewellers' gold.
Professor Haughton.				
("Phil. Trans.," 1863, p. 81).....	75 37	..	47 47	
G. Quincke.				
("Pogg. Jubelband," p. 336).....	72 47	..	43 12	for C line.

Assuming that the tangent of the angle of principal incidence is the index of refraction of the metal for red light, the value of that angle in air, as deduced from the measurements made in water and carbon bisulphide with the quarter undulation plates, is 76·53 and 77·22 instead of 76°.

The numbers given by Quincke ("Pogg. Ann.," vol. cxxviii, p. 541) for silver are—

	Principal incidence.		Principal azimuth.
	° ′		° ′
In air.............	74 19	....	43 48
In water..........	71 28	....	44 03
In turpentine......	69 16	....	43 21

The value for the principal incidence in air calculated according to the same assumption, by multiplying the tangent of the principal incidences in water and turpentine by the refractive indices of these substances, is 75° 55′ and 75° 36′ instead of 74° 19′; in all four cases the value is too high.

Although more experiments are required to decide this point, it seems probable that this relationship between these numbers is not merely an accidental one; and if so that there is additional reason for adhering to Sir David Brewster's opinion that the value of the angle of principal incidence may be taken as indicating the refractive power of a metal.

In conclusion, I must express my thanks to Professor Stokes for much advice and assistance, and specially for pointing out the method for determining the residual corrections to the results obtained with the quarter undulation plates.

January 16, 1879.

W. SPOTTISWOODE, M.A., D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On some Points connected with the Anatomy of the Skin." By GEORGE THIN, M.D. Communicated by Professor HUXLEY, Sec. R.S. Received November 25, 1878.

[PLATES 2 and 3.]

Rollett, in 1858,* in a memoir on connective tissue, described the results of an elaborate investigation into the structure of the corium. Microscopic examination of leather, and of skin tanned by himself, had shown him that the connective tissue bundles of the corium are made up of smaller divisions, and that these latter are again made up of the previously known minute connective tissue fibrillæ, which are so small that their diameter can only be approximately estimated at 0·0002 to 0·0003 millim. From the connective tissue bundles of the skin of the ox, "treated by lime or baryta water, there can," he states, "be isolated from each bundle a number of component elements which have a considerably larger diameter than the minute fibres known as connective tissue fibrillæ." These elements have, he remarks, in the ox a thickness of 0·003—0·006 millim., and he proposes to call them connective tissue fibres (*Bindegewebsfaser*). In a plate attached to his memoir the bundles and their divisions are shown in a very distinct manner.

This observation of Rollett's has not arrested the attention of anatomists to the degree which might have been expected, and seems, indeed, to have been to a great extent neglected. Two of the latest standard works may be quoted in illustration of this remark. W. Krause, in a volume on "General and Microscopic Anatomy," published in 1876, describes the tissue of the corium proper as being composed of "a network of strong bundles of connective tissue closely interwoven, the bundles being partly cylindrical, partly flattened." There is nothing said about the subdivision of the bundles, as described by Rollett.

The same author, in his chapter on connective tissue, states, "that the ground substance of fibrous tissue consists of closely-packed, very fine, round connective tissue fibrillæ, measuring 0·0002—0·002 millim." The larger of these measurements is inapplicable to the fibrilla of Rollett, and is so near that of the subdivision or "fibre" of that author, that it is evident that Krause does not recognise the distinction between the fibre and the fibrilla established by the former histologist.

In Quain's "Anatomy"† it is stated, "that the corium is made up of an exceedingly strong and tough framework of interlaced connective tissue fibres with blood-vessels and lymphatics. The fibres are

* "Sitzungsbericht der Kaiserlichen Akadamie der Wissenschaften," vol. xxx.

† Eighth edition, edited by Dr. Sharpey, Dr. A. Thomson, and Mr. Schäfer; p. 213, vol. ii.

chiefly of the white variety, such as constitute the chief part of the fibrous and areolar tissues, and are arranged in stout interlacing bundles, except at and near the surface, where the texture of the corium becomes very fine." Neither in the above quotation, nor in the sections of the same volume in which areolar tissue and fibrous tissue are described, can I find anything analogous to Rollett's description of definite subdivisions of the bundles as distinct from the fibrillæ.

In a paper presented to the Royal Society in 1875, I stated that in portions of the cutis, macerated for a few days in aqueous humour or blood serum, the tissue is seen to be composed of extremely fine but sharply contoured fibrillæ, arranged in parallel bands, whose breadth approaches the diameter of a human red blood-corpuscle. These bands are the subdivisions (*Abtheilungen*) of the bundles described by Rollett, with whose memoir I was not then acquainted.

During the interval that has elapsed since I wrote the paper referred to, I have been frequently engaged in examining skin affected by various pathological changes, and I have had occasion to observe that the structure of the "bundle" of anatomists, as understood by Rollett, is sometimes seen very clearly in disease. Its recognition is, as I have elsewhere* pointed out, necessary to a right appreciation of some of the appearances seen in cancer of the skin.

It is partly the object of this paper to describe some methods by which this structure of the bundles can be demonstrated, and also to describe some other points in the anatomy of the skin which I have observed whilst studying the tissue by means of these methods.

The nomenclature I shall use is the following: By the term *bundle* or *secondary bundle*, I designate the ordinary bundle of authors, which is more or less conspicuous in all preparations of skin, and which is analogous in structure and size to the bundles as usually described and figured in tendon-tissue. The element described by Rollett as "connective tissue fibre," I shall describe as *primary bundle*, to distinguish it more markedly from the fibrillæ which compose it.

When groups of secondary bundles are isolated, each group being composed of several secondary bundles, I term the group a *tertiary bundle*.

These elements can be isolated by first saturating the corium with chloride of gold solution and then macerating the tissue in acids. Portions of skin, with a thick layer of the panniculus adiposus, were taken fresh from the mamma of a middle-aged woman, which had been removed for a tumour of the gland—the portions of skin chosen being well clear of diseased tissues. The stretched skin was pinned down to a cork board, the under surface uppermost, and then saturated

* "Trans. Roy. Med. Chir. Soc.," vol. lix, p. 189.

with half per cent. chloride of gold solution. From time to time different thicknesses of the fatty layer were removed as the solution had had time to penetrate into the tissue, until finally the deeper layer of the cutis proper was laid bare. The tissue, still extended, was then placed in fresh gold solution for several hours. The object of the manœuvre was to secure the penetration of the fluid through the bundles, whilst these were still extended in their natural condition.

After a due action of the gold, the skin was cut into small pieces, which were then treated by acetic and formic acids in various degrees of dilution. Some of the portions were exposed to sunlight for several days, in water feebly acidulated with acetic acid, and then the strength of the acetic acid was raised to 20 per cent. of the ordinary concentrated acetic acid of commerce. Other portions were treated by formic acid. Some successful preparations were obtained from portions macerated first for a few days in a mixture of one part formic acid, of specific gravity 1·020, and one of water, and then in the undiluted acid for some days longer, but a strict adherence to these strengths was not found necessary.

Portions of the corium thus prepared were teased out in glycerine and examined, directly or after staining by different dyes. Staining by pikric acid I found very advantageous.

I was able to isolate, in a condition favourable for study, the primary, secondary, and tertiary bundles. Generally speaking, although not invariably, the tertiary and secondary bundles were best seen in the tissues macerated in acetic acid, and the secondary and primary bundles in those treated by formic acid.

Numerous elastic fibres were isolated by both methods, the finest fibres more particularly in the formic acid preparations. These fibres were frequently found only partially detached from the bundles, and in such preparations the relations of the fibres to the bundles could be well studied. The primary bundles isolated by these methods were flattened, cylindrical elements, even contoured, homogeneous in appearance, and uniform in breadth over the whole length isolated. The difference in breadth between individual bundles was very slight. By measurement, I found that they were from 0·004 to 0·005 millim. broad. The primary bundles were sometimes seen *in situ*, that is to say, as parts of a secondary bundle, the breadth of the latter being normal. In other preparations the contours of secondary and tertiary bundles were lost, and the microscopic field was filled with a large number of primary bundles, entangled and twisted by the needles used in teasing them out. Sometimes a number of primary bundles, although separated from each other, were yet so placed that I could feel assured that they were the constituent elements of one secondary bundle. Such was the case with the primary bundles shown in fig. 4.

Various methods have been recommended by histologists for the demonstration of the ultimate fibrillæ of fibrous tissue, chiefly with reference to those of tendon bundles.

If I may judge by my own preparations of skin and by the figures published in histological works, the fibrillæ of the cutis bundles are very seldom seen. The appearances usually observed in skin hardened by chromic acid and alcohol are unfitted for a study of the fibrillæ. In such specimens the bundles are more or less broken up, but the individual fibrillæ are not, as a rule, isolated.

I found that they were well shown by the following method:—A portion of fresh skin, with the panniculus adiposus attached, was pinned to a piece of cork, in the manner already described, and treated in the same way, with the exception that this time glycerine, instead of chloride of gold solution, was used for saturation. When the saturated cutis tissue had been laid bare, the whole was placed in glycerine and allowed to remain in it for several days. Small portions were then teased out in glycerine, stained by picro-carminate of ammonia and examined in glycerine. In such preparations the secondary bundles were found isolated, the contours of the primary bundles not being preserved. In the secondary bundles the fibrillæ were seen more or less distinctly, in some of them with perfect distinctness. (See fig. 8.)

In the gold preparations the following facts regarding the disposition of the elastic fibres were noted:—

If a portion of skin is hardened in bichromate of potash, and the sections moderately stained by eosin, all the large elastic fibres are stained much more intensely than the bundles, and it is then observed that they lie on the surface of the bundles, and run parallel to them. In the gold preparations, after maceration in formic acid, further details regarding the fibres can be detected. It is then seen that there is a close network of minute elastic fibres, of which I have observed no traces in eosin-stained bichromate preparations, on the surface of the bundles, and that at certain points the larger fibres give off branches which join this network. At these points the network is so dense over a small defined space that the size of the meshes is nearly equalled by that of the fibres.

Rollett, in the memoir referred to, states that the bundles are embraced by elastic fibres, and that the latter send branches into the substance of the bundles. I am able to confirm this statement, and to extend it. In some of the gold and formic acid preparations, I have observed that the elastic fibres which penetrate the bundles enter between the primary bundles, and that the primary bundles are embraced by the fibres which entwine them very closely. I have never observed an elastic fibre penetrate a primary bundle.

The relation of the elastic fibres to the primary bundles is shown in

fig. 7, but the fibres are in reality more delicate than is shown in the drawing.

The dark very finely granular deposit produced by the reduction of the gold chloride had a special relation to the elastic fibres, which was best observed in portions of skin which had been macerated for a longer period in 20 per cent. acetic acid. This relation will be understood by reference to figs. 3, 6, 9, and 10. Strictly defined narrow strips of this deposit were found investing the fibres, and this so closely that it was only at points where it had been disturbed in the preparation that the fibre itself could be observed.

The appearances reproduced in figs. 3 and 10, in which fibres are seen with deposit still adherent, illustrate this point very strikingly.

In gold preparations large flat oval nuclei are sometimes seen adherent to the surface of the bundle. The nuclei have the characteristic slate colour, and around the nucleus a small ill-defined patch of gold deposit is seen.

This deposit could sometimes be seen to be continuous with that surrounding an elastic fibre. This is shown in fig. 10. There is no reason to believe that in such cases more of the cell than the nucleus has been preserved, or that the gold deposit has any special relation to cellular substance.* The distinctly localised character of the deposit around the elastic fibres supports the idea that the larger ones are surrounded by an albuminous fluid, of a like nature to that shown by gold preparations, to be present between the laminæ of the cornea.

Isolated tertiary bundles completely surrounded by elastic fibres (fig. 5), are sometimes seen.

The "spiral" fibre, as I have seen it on the bundles of the skin, is an elastic fibre that encircles the bundles like a ring; and it may continue to do so after the ring has been detached from other fibres, none of which, indeed, may be found in the isolated bundle.

The nature of the spiral fibre is still considered by some histologists as undecided, and Ranvier regards its behaviour under picro-carminate staining as against the view that it is an elastic fibre. In the preparation drawn in fig. 8, which had been stained by picro-carminate, a typical spiral fibre was distinctly stained yellow by the pikric acid, and was not stained by the carmine, behaving in this respect exactly like any other elastic fibre.

Confirmation of Rollett's views as to the structure of the bundles is occasionally found in bichromate of potash preparations of skin. Part of one of the most demonstrative preparations of this kind

* In a paper read before the Royal Society in 1874 ("Proceedings," No. 155, 1874), I followed the view held by some histologists, that the gold deposit in such preparations is indicative of cellular protoplasm, and described and figured (fig. 13) an anastomosis of cells in the skin by means of elastic fibres. As will be observed from the remarks in the text, I now interpret these appearances quite differently.

which I have met with, has furnished the subject of the drawings in figs. 1 and 2.

The specimen is from the skin of the horse, and was thus prepared. A portion of fresh skin, free of panniculus adiposus, was hardened first in weak and then in stronger solutions of the bichromate, and treated by gum and alcohol before being cut. The deep edge of the sections—the part of the tissue that had been in direct contact with the bichromate solution—showed the structure of the bundles best.

The transverse sections of many of the bundles were cut up into a mosaic of somewhat rounded polygonal fields (fig. 1*b* and fig. 2), the measurements across each field varying from 0·0037 to 0·005 millim. Oblique and longitudinal sections of the bundles showed that these fields were sections of primary bundles. The mosaic was not equally distinct in all the bundles, even in parts where the appearance was well brought out. This varying distinctness is seen in fig. 1.

The sections of the primary bundles being rounded there are small angular spaces between them. These have not been successfully shown in the drawing.

In this preparation a delicate connective tissue was found between the bundles of the corium in a well marked form. Its extent relatively to the bundles will be best understood by reference to the drawing. As seen in the preparation it was distinctly fibrillar at parts.

The cells seen in the preparation were in two positions. Some of them were found in the delicate tissue between the bundles; other cells were found in direct connexion with the bundles. Of the latter cells the greater number seen were applied to the surface of the bundles, but others were found in the substance of the bundles between the primary bundles.

These cells were all of the endothelial type. In all of them the cell-contour was clearly marked, and in none of those observed was there a trace of a process, or of ridges and depressions similar to those described by some histologists in tendon. The size and form of these cells is accurately shown in fig. 1, and will be better appreciated by reference to the drawing than by any detailed description which I could give.

EXPLANATION OF THE PLATES.

(All the figures except figs. 8 and 10 are drawn by camera lucida.)

Figure 1. From the corium of the horse, bichromate of potash, gum, and alcohol. Logwood and eosin staining.

(*a*.) Delicate connective tissue between the bundles.

(*b*.) Secondary bundle cut transversely, showing mosaic formed by the sections of primary bundles.

(*c*.) Cells belonging to the inter-fascicular connective tissue.

(*d*, *f*.) Cells lodged in spaces in the centre of bundles.

(*e*.) Cell applied to the surface of a bundle. × 375.

Figure 2. Part of a bundle hanging loosely on the free under edge of the same section from which fig. 1 is drawn. The mosaic of primary bundles is unusually well marked. × 375.

Figure 3. Elastic fibre, with patches of chloride of gold deposit adherent. Isolated from adult human skin. Gold saturation and maceration in 20 per cent. acetic acid. × 340.

Figure 4. Isolated primary bundles. Human adult skin. Gold saturation and maceration in formic acid. × 340.

Figure 5. Tertiary bundle entwined by elastic fibres. Human adult skin. Gold saturation; maceration in acetic acid. × 340.

Figure 6. Gold deposit on a large elastic fibre, and a small elastic fibre on the surface of a bundle almost completely ensheathed in gold deposit. Human adult skin. Gold saturation; maceration in acetic acid. × 340.

Figure 7. An isolated secondary bundle, in which the contours of the primary bundles are visible. The latter are entwined by minute elastic fibres. Human adult skin. Gold saturation: maceration in formic acid. × 340.

Figure 8. Bundle showing fibrillæ, and snared by an elastic fibre (spiral fibre). Human adult skin. Saturation with glycerine; picro-carminate staining. (Hartnack, Objective No. 8; Eye-piece No. 3; Tube in.)

Figure 9. Lines of gold deposit on bundles, following the course of elastic fibres. Human adult skin. Gold saturation: maceration in acetic acid. × 340.

Figure 10. Elastic fibre with a nucleus adhering to it, and a streak of gold deposit partially detached from the fibre. (Hartnack, Objective No. 8; Eye-piece No. 3.)

II. "On Hyaline Cartilage and deceptive appearances produced by Reagents, as observed in the examination of a Cartilaginous Tumour of the Lower Jaw." By George Thin, M.D. Communicated by Professor Huxley, Sec. R.S. Received November 25, 1878.

[Plate 8.]

The following paper is written with a twofold object: firstly, as a contribution to the histology of hyaline cartilage; secondly, to illustrate how much the apparent structure of a tissue which is being examined microscopically depends on methods of preparation.

A portion of a large tumour of the lower jaw, believed from its naked eye appearances by two experienced surgeons to be sarcomatous in its nature, was given me for examination. Although I was struck by the peculiar kind of resistance it offered to the knife, I did not imagine at the time, any more than did the surgeons who excised it, that the tumour was cartilaginous. This is to be explained by the fact that the cartilaginous substance which had been growing with extreme rapidity was of a low type.

In order to determine the structure of the growth, I hardened por-

tions of it in different solutions, and then made sections which I stained with various colouring agents. The sections thus prepared differed from each other in a remarkable manner.

Sections from a portion which had been placed for two days in solution of bichromate of potash were stained by logwood, picro-carminate of ammonia, and eosin respectively. In all of them the ground substance of the tumour appeared as structureless, and throughout it were interspersed a large number of rounded nuclei. In the carmine-stained preparations many of the nuclei were immediately surrounded by this homogeneous substance, without any appearances of what might have been considered as cell-substance or as cell-processes being observed. In some instances a scant, faintly granular stained substance tapered for a very short distance from opposite poles of the nucleus, producing the appearance of a spindle or fusiform cell. More rarely a long slender stained projection tapered gradually from one of the poles of the nucleus to a considerable distance, and seemed to end in a fine colourless fibre. The appearances were such as have been often described as indicative of cells with branching protoplasmic processes. For example, some of these apparent cells resembled accurately the smaller of the coloured figures described by Ranvier in the omentum as "vaso-formative cells."

The sections stained in eosin solution showed somewhat the same appearances, although in a more exaggerated form. A homogeneous unstained ground substance was permeated by process-like prolongations of a finely granular stained substance which surrounded the nuclei, the prolongations from adjoining cellular centres anastomosing. The distribution of these cell-like masses of stained matter was an exact copy of the appearances seen in a cornea stained by gold chloride when what has been called the "positive image" is successfully produced, and would certainly quite recently, if not now, have been described by some histologists as a highly developed protoplasmic network of branching cells (fig. 14).

In the logwood sections the nuclei alone were stained, but stretching in various directions from the nucleus strong tapering colourless fibres appeared to be given off. A system of branched cells, in which the protoplasm was very scant, and the processes highly developed, was exactly simulated. This appearance was the more deceptive, as when the tissue was broken up with needles, numbers of these apparently branched cells with broken processes were found free in the fluid (fig. 13).

Slices of the tumour had been placed fresh in solution of purpurine (Ranvier's formula), and had been allowed to remain in it for several days. The surfaces of the slices were well stained, but the colouring action of the dye had not penetrated deeply. Thin sections were made from the stained surfaces and examined in glycerine. The

nature of the tumour was at once apparent. Instead of the homogeneous ground substance seen in the other preparations, a typical hyaline cartilage, with a large proportion of so-called cartilage cells, was brought into view. The nuclei were well stained, but the cartilage substance proper was only very faintly coloured. In every part of all the purpurine sections the cartilage structure was perfect (fig. 11).

The purpurine solution contains one-third per cent. alum, and one-fourth its bulk of methylated alcohol; and it is to this composition probably more than to the staining that the preservation of the unstable cartilage substance was due.

Portions of the tumour which had been placed in half per cent. solution of osmic acid were teased out, stained in logwood and examined in glycerine. Indications of the cartilaginous structure could be detected, but the preparations were chiefly valuable as demonstrating the nature of the cells. The cell-substance was stained a darkish brown colour, the nucleus was well stained by logwood, and the ground substance was very feebly stained. The outlines of the cells could thus be observed *in situ*, as well as studied in isolated cells, many of the latter floating free in the fluid.

All the cells observed were flattened, rounded, or somewhat polygonal bodies, with round nuclei (fig. 12). Their contours did not correspond exactly with those of the rounded cartilage "capsules" in which they lay.

In order to study the structure of these so-called "capsules," portions of the purpurine preparations were broken up. Considerable fragments with even surfaces were thus obtained, with rounded nuclei on the surfaces. In some of these there was no trace of the capsule formation. In other fragments a long piece of cartilaginous ground substance gave off laterally small curved projections, the size of the projection and degree of the curvature showing that they formed parts of a capsule. But in no instance was an entire capsule isolated. On the other hand, a curved projection could sometimes be traced round one side of a capsule, encircling nearly one-half of it, and then passing onwards to form the bent wall of another capsule. I never observed these projections doubling back round the capsule (fig. 15).

The examination of this tumour has thus shown that most delusive appearances as regards the nature of cartilage cells may be sometimes produced by staining and hardening agents. Carmine and eosin by staining an unformed substance that exists in the structure in defined tracts, may simulate branched protoplasmic cells, and bichromate and logwood preparations, either in sections or teased out, may as closely simulate cells with fibre processes.

These facts justify serious doubts as to the correctness of interpretation in all cases in which histologists have described branched cells

in hyaline cartilage, whether the latter existed as a normal structure, or as a pathological growth. They further show that, taken alone, carmine or eosin staining should not be held as conclusive evidence of the existence or limits of cellular protoplasm in any animal tissue.

EXPLANATION OF THE PLATE.

(All the figures are drawn by camera lucida; magnifying power × 260.)

HYALINE CARTILAGE.

Figure 11. The normal structure of hyaline cartilage. Purpurine.

Figure 12. Isolated cells. Osmic acid.

Figure 13. Isolated nuclei adherent to portions of cartilage substance, simulating branched cells with fibre-processes. Bichromate of potash; logwood.

Figure 14. Stained substance in the cartilage simulating branched cellular protoplasm. Bichromate of potash; eosin.

Figure 15. Fragments of cartilage substance separated by needles. Purpurine.

III. "Volumetric Estimation of Sugar by an Ammoniated Cupric Test giving Reduction without Precipitation." By F. W. PAVY, M.D., F.R.S. Received December 5, 1878.

To be able to effect the quantitative determination of a body with accuracy and facility is an important matter looked at in relation to the study of its bearings. In the case of sugar there are no reliable means of precipitating and weighing it, either alone or in combination, and thus in the chemical estimation of this principle an indirect method has to be resorted to. The only property upon which dependence can be placed, for the purpose of chemical quantitative analysis, is its reducing action, under the influence of heat, upon certain metallic oxides, and that of copper is the one which general experience shows to answer best.

In the ordinary volumetric application of the copper test, the precipitation and diffusion of the reduced suboxide through the liquid interferes with the clear perception of the precise point of complete decoloration, and thus detracts from its delicacy. For purposes where minute accuracy is of no moment, a sufficiently approximate result can be obtained, but for physiological investigation, and in other cases where precision is indispensable, the process is quite unfit for employment.

With the view of obtaining increased accuracy, chemists have had recourse to the plan of collecting the precipitate of reduced suboxide and weighing it as such or after reconversion into the oxide. From the difficulty, however, that exists in procuring the metallic oxide in a pure and uniform state, and from the impossibility of completely freeing the filter paper used from adhering surplus copper solution, some uncertainty is given to the results obtained by this method. To

Fig. 1

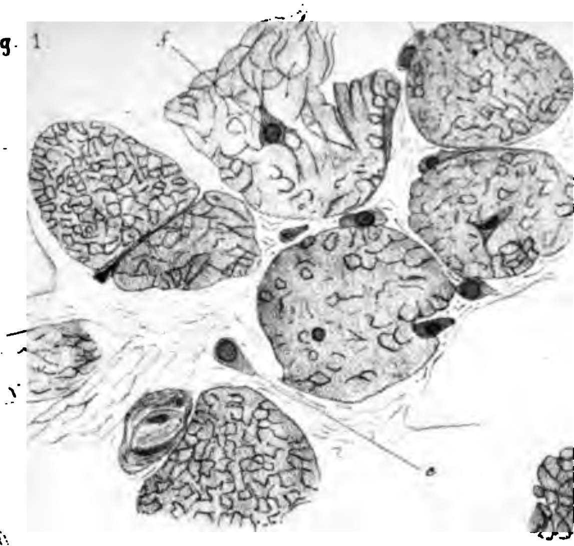

Fig. 2.

Fig. 3

Fig. 4.

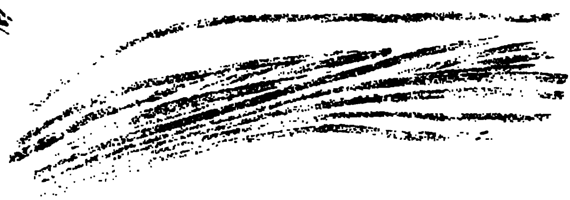

Fig. 5

Fig. 6.

G. Thin ad nat. del.

West, Newman & Co. imp.

[redacted]

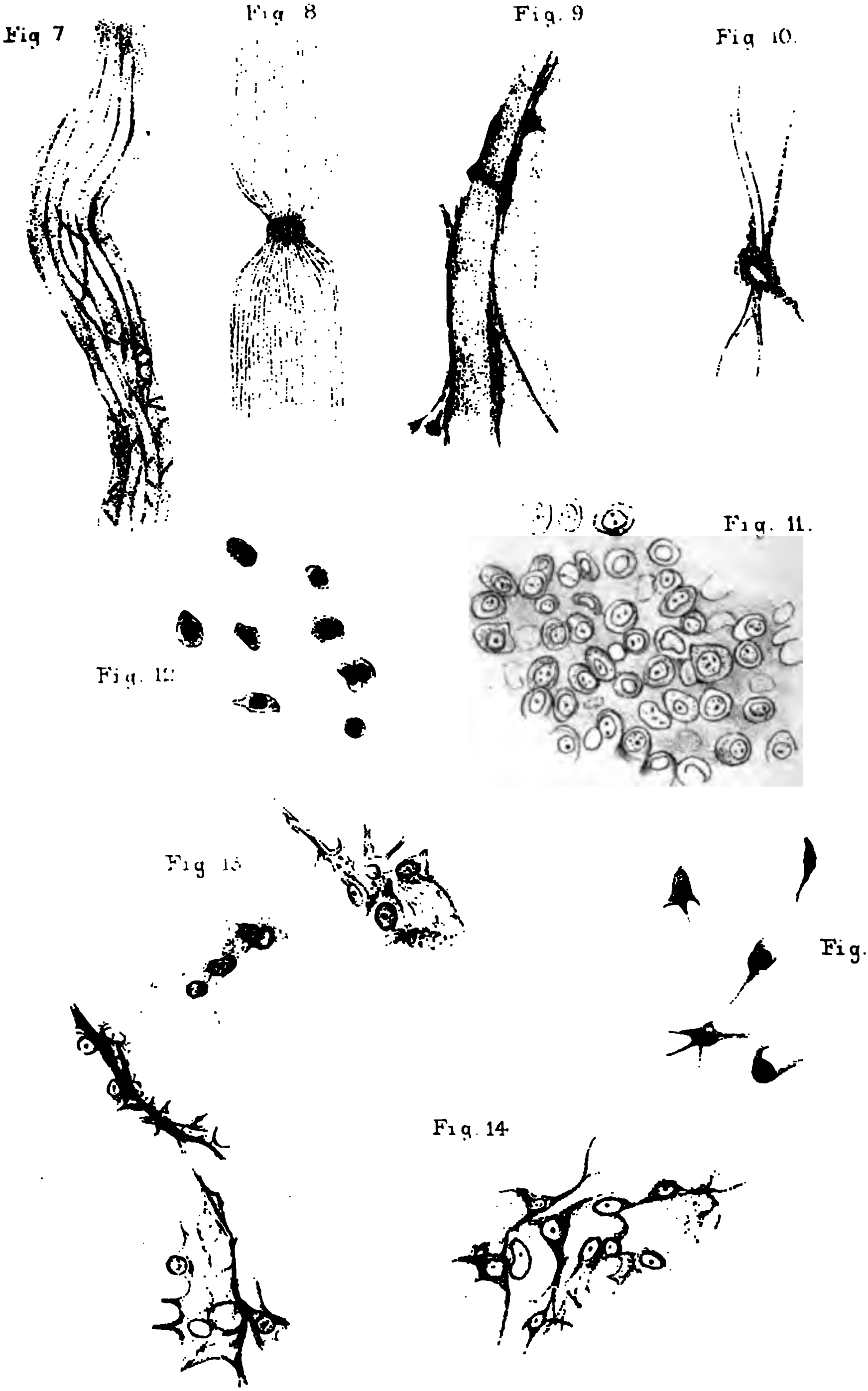
Fig 7
Fig 8
Fig. 9
Fig 10.
Fig. 11.
Fig. 12
Fig 13
Fig.
Fig. 14

obviate the difficulty here presented, I suggested, in a communication published in the "Proceedings of the Royal Society" for June, 1877, that the precipitated suboxide should be collected and dissolved, and the copper subsequently thrown down by the agency of galvanic action upon a platinum cylinder, as is now frequently done in the assaying of copper ores. The process has been found, as shown by the closeness observable in the results of counterpart analyses, to admit of the greatest precision, and I have turned it to extensive account in some recent physiological investigations I have conducted. In its application to such a purpose, it may be held that time and labour should be considered as of no moment, but it frequently happens that a more ready process of estimation is needed than the gravimetric supplies, and on this account a volumetric method, free from the objection I have pointed out as belonging to the ordinary plan, constitutes a desideratum.

A few years back Bernard introduced, for physiological purposes, a modification of the ordinary volumetric process, which is attended with reduction and the non-precipitation of the reduced oxide. The process involves the employment of a large quantity of caustic potash, and the presence in the product to be tested of extraneous organic matter. Under these circumstances it happens that the reduced suboxide is held in solution instead of being allowed to fall, and thus decoloration without precipitation occurs and enables the point of disappearance of the colour of the test to be ascertained with precision. Bernard, in his remarks upon the test, simply made mention of the fact that under these conditions, reduction without precipitation took place, but Dr. d'Arsonval,* his *Préparateur* at the College of France, refers the effect to the solvent influence of the extraneous organic matter in presence of the alkali.

Whilst engaged upon an inquiry into the merits of this test, the conclusion suggested itself to me that the agency preventing the deposition of the suboxide was the development of ammonia. With an absolutely pure solution of sugar, such as may be obtained by inverting the ordinary crystallized cane sugar (refined loaf sugar) no amount of potash will hinder the instantaneous precipitation of the suboxide. With commercial grape sugar, however, and in a still more marked manner with honey, interference with precipitation is temporarily exerted, and this, I am led to conclude, is due to the action of the potash in producing ammonia from the small quantity of nitrogenous organic matter incidentally present.

With this before me, the idea presented itself of resorting to the direct employment of ammonia for attaining the same result. It is well known to chemists that ammonia is a powerful solvent of the suboxide of copper, leading to the production of a perfectly colourless

* "Gazette Hebdomadaire de Médecine et de Chirurgie," Sept. 14, 1877, p. 454.

liquid; and this, from the facility with which it absorbs oxygen, quickly assumes a blue colour under exposure to air from the reconversion of the cuprous into the cupric oxide.

If ammonia be added to the ordinary Fehling's solution, a liquid is obtained which is rendered colourless by boiling with a sufficiency of sugar to effect the complete reduction of the cupric oxide present to the state of suboxide. As the saccharine product is dropped in the blue colour gradually fades, without any occurrence of precipitation to interfere with the perception of the precise moment when the point of complete decoloration is attained. The ammonia exerts no interference with the process of reduction, but simply dissolves the reduced oxide, leading, when complete decoloration is effected, to the production of a perfectly colourless, limpid liquid.

Enough ammonia must be present to secure that the suboxide is held in solution, and precaution must be taken that whilst the analysis is being performed the reduced oxide does not become reconverted into the oxide by exposure to the air. To obviate this the operation should be conducted in a flask instead of an open capsule.

The appliance that naturally suggests itself as most suitable for employment is a flask of about 80 cub. centims. capacity, with a cork inserted into the neck, through which a delivery tube from a Mohr's burette, graduated in tenths of a cub. centim., passes for dropping in the product to be examined. Through the cork, also, there must be an exit tube for the escape of air and steam from the flask. Should it be desired to avoid the impregnation of the surrounding atmosphere with ammonia, the exit tube may be connected by vulcanized tubing with a U-shaped tube containing fragments of pumice stone moistened with water or a weak acid. The burette being fixed in the stand, the flask is allowed to hang suspended, so that there may be nothing to obstruct the full view of its contents. The heat is applied by means of the flame of a spirit lamp, and the best position for watching the disappearance of colour is by the light reflected from a white background specially provided for the purpose. It is convenient to have another burette, graduated in cub. centims., and of 100 cub. centims. capacity, fixed in the stand for holding and delivering the ammoniated copper solution. Messrs. Griffin, of Garrick Street, have constructed an arrangement adapted to meet the requirements.

I at first took it for granted that in the action occurring the same relation existed between the amount of oxide of copper reduced and that of sugar oxidised, as under the employment of the copper test in the ordinary way, viz., that 5 atoms of oxide of copper were reduced by 1 atom of sugar, and the liquid I first employed was prepared by adding to 100 cub. centims. of Fehling's solution 300 cub. centims. of strong solution of ammonia (sp. gr. ·880) and 600 cub. centims. of distilled water. The liquid thus made contained one-tenth of

Fehling's solution, and if it comported itself in the same manner as the latter, 10 cub. centims. of it would stand equivalent to ·005 grm. of grape sugar. In working with this liquid the results obtained were so accordant in relation to each other that I had no misgiving about its uniformity of action; but I felt that before being definitely accepted they ought to be checked against known amounts of sugar. The accomplishment of this proceeding, however, is not altogether unattended with difficulty, on account of the uncertainty of obtaining grape sugar free from impurity and in a perfectly dried state.

The method I have adopted has been to operate upon weighed amounts of cane sugar and produce inversion by boiling with an acid. I first found that the cane sugar, which is sold in coarse colourless crystals—that which is known as "white crystal," and used for sweetening coffee—stood the test on examination for purity with Laurent's polarimeter. A weighed quantity was taken, and, after being inverted by boiling with hydrochloric acid, the acid neutralised, and the liquid brought to a known volume, subjected to treatment with the ammoniated copper liquid. Repeated trials were made with varying quantities, and it was found that the results stood in harmonious relation to each other, but that the amount of sugar indicated was larger than the calculated amount of invert sugar from the weighed quantity of cane sugar taken. At first I was at a loss for an explanation of this result, but subsequent observation has revealed that in the case of the ammoniated liquid, 6 atoms of oxide of copper are appropriated by 1 atom of sugar, instead of 5, as in that of Fehling's solution used in the ordinary way. When the reckoning is made upon this basis the results exactly correspond with the actual amount of sugar known to be present. Moreover, with solutions of ordinary grape sugar and diabetic sugar, examined comparatively with Fehling's solution used in the ordinary way and the ammoniated copper liquid, the results exactly accord under the reckoning that 5 atoms of oxide of copper are appropriated in the one case and 6 atoms in the other by 1 atom of sugar.

To be quite satisfied upon this point, a large number of observations under varying conditions have been made, and whilst what I have stated holds good for the ammoniated copper liquid prepared from Fehling's solution, without any further addition of alkali, and with the addition of potash to the extent of 1 grm. to 20 cub. centims. of the ammoniated test, yet a larger quantity of potash alters the action, and with 5 grms., and anything beyond, the behaviour is brought to the same as that of Fehling's solution used in the ordinary way, viz., 5 atoms only of oxide of copper are appropriated by 1 atom of sugar. With quantities of potash between the 1 and 5 grms., the results stand between the 5 and 6 atoms of cupric oxide.

I may mention that observation has further shown that whilst

glucose prepared from starch behaves like other varieties of grape sugar, there is an intermediate product formed before the completion of the process of conversion, which behaves in a different manner from invert sugar, grape sugar, and sugar of diabetes. Estimations made with the ammoniated copper liquid coincide with those made with Fehling's solution without the presence of ammonia, and the addition of potash to the ammoniated liquid produces no modification of the result.

In order that the ammoniated copper liquid may be brought to the same standard of sugar value as Fehling's solution, and it is desirable that this should be the case, the proportion of copper must be increased so as to give 6 atoms against 5. By taking 120 cub. centims. of Fehling's solution, 300 cub. centims. of strong ammonia (sp. gr. ·880) and making up to a litre with distilled water, the proper proportion is obtained, and the ammoniated liquid gives results corroborated in accuracy by the balance, and coinciding with those obtained by Fehling's solution employed in the ordinary way.

As a minor point it may be remarked that the diluted state presented by the ammoniated liquid offers an advantage by diminishing the liability to error arising from any want of absolute precision in measurement.

Twenty cub. centims. of the ammoniated copper solution, corresponding with ·010 grm. sugar, having been run in from the burette containing the test, the flask is adapted to the cork attached to the delivery tube of the other burette containing the saccharine product for examination. The flame of a spirit lamp is then applied underneath, and the contents of the flask brought to a state of ebullition and allowed to boil for a few minutes in order to get rid of the presence of air. The saccharine product is now allowed to drop from the burette until the blue colour of the test is just removed, and a perfectly colourless limpid state produced.

On account of the ammoniated copper solution used being only equivalent to 2 cub. centims. of Fehling's solution, it is necessary that the product to be examined should not be in too concentrated a form. For delicate observation it is convenient that the dilution should be such as to require the employment of from about 10 to 20 cub. centims. to decolorize the 20 cub. centims. of the ammoniated copper solution.

The ammoniated copper solution enjoys the advantage of possessing a self-preservative power. It is well known in the case of Fehling's solution that, in the course of time, not only does the liquid become impaired in stability, but actually reduced in strength, by the spontaneous deposition of a certain amount of suboxide. Not so, however, with the ammoniated liquid. Here the conditions are such that *under* exposure to air the copper cannot fail to remain in solution and

to be maintained in a fully oxidized state. A further advantage is given by the influence of the presence of ammonia on the colour of the test, for, in proportion to the height of colour of a volumetric liquid, so is its degree of delicacy as a reagent, and the effect of the addition of ammonia to the ordinary copper test is to considerably increase the blue colour belonging to it.

Seeing that the test here proposed acts with equal efficiency either in the presence or absence of extraneous organic matter, it is alike adapted for employment by the chemist, the physiologist, and the medical practitioner in relation to diabetes.

IV. "On the Effect of Strong Induction-Currents upon the Structure of the Spinal Cord." By WILLIAM MILLER ORD, M.D., F.L.S., Fellow of the Royal College of Physicians, Physician to St. Thomas's Hospital. Communicated by J. SIMON, C.B., D.C.L., F.R.S. Received December 17, 1878.

(Abstract.)

The results of a series of experiments are related. They were founded upon considerations offered by chorea, tetanus, and similar diseases; certain clinical facts and post-mortem observations having led the author to suppose that the occurrence of protoplasmic convulsion or spasm in the grey matter of the nervous system was consistent with the morbid appearances and with the history of cases.

The present series of observations was made upon adolescent dogs. The spinal cord was the part selected for experiment. The dogs were killed by chloroform, and the cord, rapidly exposed, was galvanized for different periods and in different directions. In all cases parallel experiments were made with dogs of the same age and size, all points of the operation being carried out in the same way, save for the application of the galvanic currents.

The following effects were observed:—

1. Broadening of the cord in parts through which currents had been passed longitudinally, narrowing where transverse currents had been applied.

2. In the narrowed parts a great diminution in the sectional area of the grey matter with retraction of the posterior horns.

3. In the same parts a remarkable dilatation of the central spinal canal, and an infiltration of myelin and leucocytes into the cavity.

4. The production of spaces around corpuscles, vessels, and nerve-bundles by the retraction of the protoplasmic matter. Such spaces were often found filled with *débris*, containing coagula, myelin, and vacuoles. They corresponded in appearance with the "perivascular erosions" of Dickinson.

5. The contraction of nerve-corpuscles, which, being much more marked between their branches, gave them a scalloped appearance. Vacuoles were formed within them, and in the spaces formed by their retraction, and by the retraction of surrounding parts.

6. In some places rupture of nervous tissue was observed.

7. In longitudinal sections nerve-fibres were found flattened and varicose, the flattening resembling that described by Elischer in fibres of median nerve in chorea.

Conclusions.—1. That, in young dogs, the protoplasmic constituent of the grey matter contracts *en masse* under the influence of strong faradaic currents.

2. That it contracts unequally and irregularly by reason of its unequal and irregular sectional area, causing thereby condensations at certain points—notably in the anterior horns and around the central canal—and rarefaction at others—notably in the middle of each crescent; such rarefaction going on sometimes to rupture of tissues.

3. That nerve-corpuscles contract in various degrees according to the strength and duration of currents, and that while they tend in contraction to become spherical they also tend to become vacuolated.

4. That the vessels are in some places strongly contracted and empty; in others dilated and filled with blood clot, having the appearance of embolus.

5. That the appearances correspond so decidedly with appearances in chorea and tetanus as to give ground for the supposition that contractions, such as are produced by electricity, do actually occur during life under the effect of nervous shock, and may be phenomena causal or associate of disease.

V. "Concluding Observations on the Locomotor System of Medusæ." By GEORGE J. ROMANES, M.A., F.L.S. Communicated by Professor HUXLEY, Sec. R.S. Received December 30, 1878.

(Abstract.)

The principal bulk of the paper is devoted to a full consideration of numerous facts and inferences relating to the phenomena of what the author terms "artificial rhythm." Some of these facts have already been published in abstract in the "Proceedings of the Royal Society" (vol. xxv), and to explain those which have not been published would involve more space than it is here desirable to allow. The tendency of the whole research on artificial rhythm, as produced in various species of Medusæ, is to show that the natural rhythm of these animals (and so probably of ganglio-muscular tissues in general) is due, not exclusively to the intermittent nature of the ganglionic

discharge, but also in large measure to an alternate process of exhaustion and restoration of excitability on the part of the responding tissues—the ganglionic period coinciding with that during which the process of restoration lasts, and the ganglionic discharge being thus always thrown in at the moment when the excitability of the responding tissues is at its climax.

Light has been found to stimulate the lithocysts of covered-eyed Medusæ into increased activity, thus proving that these organs, like the marginal bodies of the naked-eyed Medusæ, are rudimentary organs of vision.

The polypite of *Aurelia aurita* has been proved to execute movements of localization of stimuli, somewhat similar to those which the author has already described as being performed by the polypite of *Tiaropsis indicans.*

Alternating the direction of the constant current in the muscular tissues of the Medusæ has the effect of maintaining the make and break stimulations at their maximum value; but the value of these stimulations rapidly declines if they are successively repeated with the current passing in the same direction.

In the sub-umbrella of the Medusæ waves of nervous excitation are sometimes able to pass when waves of muscular contraction have become blocked by the severity of overlapping sections.

Exhaustion of the sub-umbrella tissues—especially in narrow connecting isthmuses of tissue—may have the effect of blocking the passage of contractile waves.

Lithocysts have been proved sometimes to exert their ganglionic influence at comparatively great distances from their own seats—contractile waves, originating at points in the sub-umbrella tissue remote from a lithocyst, and ceasing to originate at that point when the lithocyst is removed. A nervous connexion of this kind may be maintained between a lithocyst and the point at which the waves of contraction originate even after severe forms of section have been interposed between the lithocyst and that point.

When the sub-umbrella tissue of *Aurelia* is cut throughout its whole diameter, the incision will again heal up, sufficiently to restore physiological continuity, in from four to eight hours.

January 23, 1879.

W. SPOTTISWOODE, M.A., D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Researches on Chemical Equivalence. Part I. Sodic and Potassic Sulphates." By EDMUND J. MILLS, D.Sc., F.R.S., "Young" Professor of Technical Chemistry in Anderson's College, Glasgow, and T. U. WALTON, B.Sc. Received October 16, 1878.

The conception of a chemical equivalent as employed in these researches corresponds to a definition first given* by one of us, viz., that the chemical equivalent of a body is that weight of it which does the unit of work. We do not therefore use the term in its ordinary sense; as, for example, when it is said that H is "equivalent" to Cl, Na, &c.

The following experiments were arranged with the view of determining the effect of potassic and sodic sulphates on the rate of formation of ammonia, when nascent hydrogen is made to act on potassic nitrate. Judging from their behaviour in other cases, it was expected that in this instance, also, their action would be one of retardation. Experiment, however, has proved the reverse, on the whole, to be true.

The extremely delicate nature of the reaction, which is liable to be spoiled by the accidental falling in of a single speck of dust, or by slight variation of temperature, or unequal exposure of the different solutions to light, rendered the attempt to measure the effect a matter of peculiar difficulty.

At first, common sheet zinc, thoroughly cleansed from grease, was placed in a solution of potassic nitrate and hydrate, and the amount of ammonia formed during periods varying from twenty-four hours to one week was measured. But the results were very irregular and unsatisfactory. Galvanic couples seemed to be established at certain points on the surface of the zinc, probably due to the presence of iron or lead as impurities. Thin zinc foil was next tried, but with little better result; neither were any alterations in the shape or disposition of the foil attended with success. Fresh experiments were also undertaken with sodium amalgam instead of zinc and potassic nitrate; but the action, though rather more uniform, was still very uncertain. It was found impossible to obtain a perfectly homogeneous solution of sodium in mercury, entirely free from sodic oxide and hydrate; and this seriously impaired the accuracy of measuring out the amalgam.

The only plan which was found to give results at all comparable with each other, was using zinc amalgam and potassic nitrate. The experiments were performed in wide-mouthed glass-stoppered bottles of cylindrical shape, having an internal diameter of 60 mm., and a total capacity of 315 cub. centims. Each bottle contained 1 grm.

* "Philosophical Magazine," [5], i, 14.

potassic nitrate, 1 grm. potassic hydrate (prepared from the sulphate by means of baryta water), and a quantity of anhydrous alkaline sulphate, varying from 0 to 1 grm.; the whole being dissolved in 150 cub. centims. of distilled water, very free from ammonia. The reagents had been carefully purified. 30 cub. centims. of amalgam, prepared by dissolving 10 grms. zinc in 10 kilogs. mercury, were then added, and the "system" was preserved from dust and light. After twenty hours, the amount of ammonia was estimated by Nessler's method. Traces of this substance were occasionally present in the solutions employed, and a corresponding correction had to be made. In every experiment, nine solutions were prepared at the same time—three free from sulphate, three containing sodic sulphate, and three potassic sulphate; and the mean of each three was taken as the true value for that particular experiment. Fifteen comparisons of each of nine solutions, arranged in this way, were made with different quantities of sulphate. The temperature was taken at the beginning, in the middle, and at the conclusion of the experiment; the temperature at night being registered by one of the automatic thermometers sold by Negretti and Zambra. Owing to the extreme delicacy of the reaction and the slight causes which suffice to interfere with it, the numbers obtained from single experiments are not sufficiently reliable to measure the precise amount of change caused by varying the quantity of alkaline sulphate. Every comparison, however, though made with a different weight of sulphate from that employed in the others, involved equal weights of potassic and of sodic sulphate; and hence the relative effect of those two bodies has been very clearly approximated to.

The following table gives a summary of the observations made:—

Grm. sulphate added.	Mean temp. C.	Grm. ammonia formed in the blank (150 c.c.).	Ratio.		
			Blank.	Experiment with sodic salt.	Experiment with potassic salt.
0·1	18·4	0·0000432	100	102·2	99·5
*0·1	14·6	0·0000135	100	99·1	88·8
0·2	13·4	0·0000424	100	100·1	103·2
0·3	16·4	0·0000459	100	79·3	79·5
0·3	14·1	0·0001200	100	103·9	102·0
0·4	15·6	0·0001350	100	105·4	109·5
*0·5	15·6	0·0000655	100	108·5	97·7
*0·5	16·3	0·0000493	100	110·1	96·6
0·6	18·4	0·0000391	100	99·5	105·1
0·6	22·6	0·0000445	100	109·7	107·8
0·7	17·1	0·0000431	100	104·8	104·8
0·8	15·2	0·0000670	100	113·8	113·2
0·9	15·4	0·0000679	100	107·1	113·4
*1·0	12·3	0·0000359	100	111·1	129·8
1·0	17·0	0·0000456	100	121·7	114·9

The ratio of the working effect of sodic to that of potassic sulphate, as calculated from the numbers given above, is 100·16 : 100, with a probable uncertainty of 1·3 per cent. This is the *mean* value, reckoned by the method of least squares, from the whole of the observations. The rejection of the four experiments marked with an asterisk, which differ somewhat widely from the rest, would give the ratio 99·53 : 100, with a probable uncertainty of 0·73 per cent.; while the probable error of a single observation would then be reduced from 5·02 to 2·4 per cent. [Owing to the number of determinations made, any error in the result is but very slightly affected by error in the ammonia estimation.]

The conclusions which we think may fairly be drawn from these numbers are:—

(1.) That sodic and potassic sulphates have a well-marked influence on the reaction to which we have referred;

(2.) That as more sulphate is added, the reaction is accelerated;

(3.) That equal weights of sodic and potassic sulphates have as nearly as possible the same working effect.

The last conclusion may be otherwise expressed thus:—

If we represent our equivalent of potassic sulphate by a number, then the equivalent of sodic sulphate is represented by the same number.

II. "Researches on Chemical Equivalence." Part II. Hydric Chloride and Sulphate. By EDMUND J. MILLS, D.Sc., F.R.S., and JAMES HOGARTH. Received December 4, 1878.

While carrying out our researches on lactin,* it struck us that use might be made of it to compare the dynamical equivalents of acid bodies. We accordingly selected hydric chloride and hydric sulphate for the measurements in question, and prepared solutions of these acids, containing respectively 73 grammes hydric chloride (2HCl), and 196 grammes hydric sulphate ($2H_2SO_4$) per litre. An experiment was first tried with 5 grammes lactin and 10 cub. centims. of the hydric chloride solution in a total volume of 70 cub. centims. At a temperature of 17° C. there was no change of rotation in twenty-four hours. In a second experiment a similar solution was raised for an hour to 40° C., and then for an hour to 60° C.; but without effect on the rotatory power. The temperature of 100° C. was finally adopted, the change at that point taking place at a rate admitting of accurate measurement. The method of experiment was as follows:— A measuring flask was made marked to contain 60 cub. centims.; in this were placed 50 cub. centims. of a 10 per cent. solution of lactin (*i.e.*, 5 grammes), the acid measured in, and the volume made up to

* *Post*, p. 273.

the mark. To prevent evaporation during heating, the neck of the flask was left long, and a narrow bent tube attached by an india-rubber joint. The time was accurately noted when the flask was placed in the bath. After half an hour, the flask was taken out quickly, plunged into cold water, and the contents when cold transferred to the polarimeter tube. The tube used in the researches on lactin had to be modified in these experiments, the cement not being able to withstand the action of the acid. In its altered form, the plate glass covers were secured by two screw rods and nuts, a thin washer of gutta-percha tissue being placed between the ends of the tube and the plates. This washer did not materially affect the length of the column, and made the tube perfectly tight. The length of the tube was thus reduced to 216 millims.

The results of these experiments are given in the following table. The quantity of acid is the only varied condition of experiment.

Action of Hydric Chloride and Hydric Sulphate on Lactin.

Total volume in each case 60 cub. centims. Weight of Lactin =5 grammes.

Half-hour intervals.	Hydric chloride.				Hydric sulphate.		
	No. 1. 4 cub. centims.	No. 2. 4 cub. centims.	No. 3. 7·5 cub. centims.	No. 4. 8 cub. centims.	No. 5. 4 cub. centims.	No. 6. 4 cub. centims.	No. 7. 7·5 cub. centims.
0	9·565	9·565	9·565	9·565	9·565	9·565	9·565
1	10·327	10·197	10·767	10·717	10·320	10·278	10·843
2	10·812	10·670	11·383	11·340	10·974	10·720	11·423
3	11·218	11·010	11·790	11·633	11·203	11·035	11·643
4	11·400	11·147	11·850	11·742	11·355	11·286	11·850
5	11·490						
6	..	..	11·852	11·754	..	11·445	11·850
7							
8	11·770	11·670			11·854	11·607	
9							
10							
11	11·765	11·715					

The equation A is deduced from the average of Nos. 1 and 2 by the method of least squares, the probable error of a single comparison of calculated and experimental numbers being ·0653.

The equation B is similarly deduced from Nos. 5 and 6, its probable error being ·0587.

C is the equation to No. 3 with a probable error ·0818
D " " No. 7 " " " ·1063
E " " No. 4 " " " ·0848

Equations.

A	$y=9\cdot6785+\ \cdot56035x-\cdot03621x^2$.
B	$y=9\cdot6500+\ \cdot63271x-\cdot04690x^2$.
C	$y=9\cdot6827+1\cdot04941x-\cdot11635x^2$.
D	$y=9\cdot7283+1\cdot00775x-\cdot11090x^2$.
E	$y=9\cdot6889+\ \cdot98951x-\cdot10931x^2$.

In each equation y is the rotation in degrees, x is the time in half-hours. By placing $\frac{dy}{dx}=0$ in each equation, we find the value of x when y has its highest value. The corresponding value of y is thence calculated by substitution in the equation considered. We thus find data for the comparison of the two acids.

HCl			H_2SO_4		
A	$x=7\cdot74$,	$y=11^\circ\cdot846$	B	$x=6\cdot74$,	$y=11^\circ\cdot780$
C	$x=4\cdot51$,	$y=12\cdot048$	D	$x=4\cdot54$,	$y=12\cdot017$
2HCl			**H_2SO_4**		
E	$x=4\cdot53$,	$y=11\cdot928$	B	$x=6\cdot74$,	$y=11\cdot780$

These results show that though 2HCl may be the "equivalent" of H_2SO_4 in weight for saturation (*i.e.*, in the ordinary sense), it certainly is not the equivalent in the dynamical sense. They also render it highly probably that HCl is equal dynamically to H_2SO_4. Ostwald,* by a method based on the alteration of the specific volume of solutions, has shown that the ratio $\frac{2HCl}{H_2SO_4}=1\cdot93$, a result which our numbers, though not as perfect as we could wish, nevertheless strongly confirm.†

* "Journ. Prakt. Chem.," N.F. xvi, p. 419.

† If the curve equations be examined, it is found that the highest value of y is practically the same in each. By taking the average value $=11^\circ\cdot924$, and calculating to specific rotation (assuming that the action involves no change of weight), the number 73·78 is obtained. This falls short of the specific rotation of galactose (83°), and seems to point to the dual nature of lactin mentioned in the researches on lactin; probably at this point the sugar in solution is Fudakowski's lacto-glucose. ("Deut. Chem. Ges. Ber.," ix, 42–44.)

III. "Researches on Lactin." By EDMUND J. MILLS, D.Sc., F.R.S., "Young" Professor of Technical Chemistry in Anderson's College, Glasgow, and JAMES HOGARTH. Received December 4, 1878.

Although lactin, or sugar of milk, has been investigated by numerous chemists, there are many problems connected with it which still await solution. We have accordingly undertaken a series of experiments in connexion with this remarkable compound, in the hope, not only of obtaining special results, but such as may be made available in studies of a more general nature. As our work throughout has been for the most part optical as well as chemical, we have first to state our methods of obtaining the constant of Jellett's polarimeter, the instrument employed in our investigations.

I. *Determination of the Polarimeter's Constant.*—*a.* By quinine sulphate. 5·5412 grms. of the sulphate were dissolved in water acidulated with hydric sulphate, and the solution made up to 100 cub. centims. The average of five readings gave a solution of $-25^{\circ}\cdot73$, equivalent to a specific rotatory power of $-232^{\circ}\cdot16$. De Gris and Alluard* give $-255^{\circ}\cdot6$, a number which is to our experimental number as 1·10096 to 1.

β. By cane sugar. Three sets of experiments on solutions containing respectively 16·3500, 8·1750, and 4·0875 grms. in 100 cub. centims., and embracing five, four, and four readings, gave a general mean reading $21^{\circ}\cdot74$, equivalent to a specific rotation $66^{\circ}\cdot48$.

This is to the generally accepted number ($73^{\circ}\cdot8$) as 1 to 1·11011.

γ. By salicin. Two sets of experiments with solutions containing respectively 4·9156 and 2·4578 grms. in 100 cub. centims., and each embracing three readings, gave a general mean reading $4^{\circ}\cdot92$, equal to a specific rotation $50^{\circ}\cdot046$. Bouchardat† gives $55^{\circ}\cdot832$, which is to the number got by Jellett's instrument as 1·11561 to 1.

The average of the three numbers, 1·10096, 1·11011, and 1·11561, gives 1·10889 as an experimental factor for converting our Jellett readings into ordinary readings.

The relation of the two scales may also be seen by examining the arc divided to read percentages of cane sugar with a solution containing 16·35 grms. in 100 cub. centims. In the Jellett instrument, an arc of $21^{\circ}\cdot666$ is divided into hundredths for this purpose; and as 16·35 grms. pure cannose read 100 on this scale, the specific rotation

* "Compt. rend.," lix, 201.

† "Compt. rend.," xviii, 298.

is 66°·256, which is to 73°·8 as 1 to 1·11386—a factor which differs from the above experimental one by 0·45 per cent.

All the specific rotations given by us are corrected by this factor, and are comparable with those in general use.

In all our experiments the specific rotation is calculated by the formula $[\alpha]=\frac{\alpha V}{lp}$. Where $[\alpha]$ = specific rotation, α = the reading in degrees, V the volume of solution containing the weight p, and l = the length of the column in decimeters (in the above experiments, 2).

II. *Determination of the Permanent Specific Rotation of Lactin.*—The lactin was purified by filtration through animal charcoal, and two or three crystallizations, after which it left no sensible residue on ignition in air. Five sets of readings were made:—

							°
(1.)	Average	of 5	readings.	Specific	rotation		52·84
(2.)	"	"	"	"	"		53·23
(3.)	"	"	"	"	"		53·37
(4.)	"	3	"	"	"		53·04
(5.)	"	"	"	"	"		53·07

The general mean of these numbers is 53°·12, which, multiplied by the factor 1·11386, gives 59°·17 as the permanent specific rotation of lactin. The number given by Berthelot* is 59°·3. In every experiment, care was taken that the rotatory power of the solution had become constant. Three different samples of lactin were employed. Experiments (1), (2), and (3), were on sample I, (4) on sample II, and (5) on sample III. As the samples were prepared at different times, and by a method varying slightly each time, the very small differences in the results show that the lactin contained little or no impurity.

III. *Examination of the Law for the Change of Rotation in a freshly prepared Solution of Lactin.*—If the rotatory power of an aqueous solution of lactin be examined at short intervals of time, it soon becomes apparent that a change is taking place, the angle through which the plane of polarization is rotated becoming gradually less. The object of the following experiments is to quantify the phenomenon in question.

Five grms. of lactin were dissolved as rapidly as possible (time taken, 1 hour 15 minutes) in cold water, and the solution made up to 100 cub. centims. The polarimeter tube (2 decims. long) was filled with the liquid, and a first observation taken 15 minutes after complete solution, or 1½ hour after first contact. Succeeding readings

* "Ann. Ch. Phys.," [3], liv, 82; lx, 98.

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Table

r. in	Containing 1 g
·8	12°·2
833	14·070
157	13·487
583	13·050
·078	12·567
·707	12 230
·330	11·935
·070	11·540
·683	11·133
·447	10·787
·213	10·547
·625	9·770
to 13°·8	11°·5 to 12°·5
6	7

3	7
9·250	8·117

were made at intervals of 2 hours, the results being given under Table I, No. 21.

See Table I.

In order to increase the total change and lessen proportionally the error of experiment, it became necessary to use a stronger solution, to increase the length of column, and to reduce the interval elapsing between first contact and first observation as far as possible. To attain these conditions the following method was adopted:—About 10 grms. of powdered lactin were rubbed in a mortar with about 60 cub. centims. of water for half an hour, the solution filtered, and the first observation taken one hour after first contact. The metal tube belonging to the polariscope was also discarded, and a glass one constructed from a piece of tubing 17 millims. wide, by sealing on a side piece for the introduction of a thermometer, and grinding the ends carefully until it measured 242 millims., the greatest length admitted by the polarimeter. Two glass disks were cemented on the extremities, and the tube covered from end to end by a helix of thin tin tubing, through which a current of water might be passed to keep the temperature constant; to guard further from variations in temperature the tube was covered with cotton wadding. With these precautions three experiments were made (Table I, Nos. 1, 2, 3), the result being that the total change was nearly doubled. In all the other experiments the method was slightly varied, the lactin being placed in a bottle with a ground glass stopper, 60 cub. centims. water placed on it, and the whole shaken vigorously at intervals for half an hour, filtered, and the first observation taken as before. Each experiment extended over six hours, and included ten observations. For each observation three or four readings were made, and the average taken. In Nos. 4, 5, 6, 7, 8, 9, 13, 14, 15, 16, 17, 18, 19, 20, of the accompanying table, varying weights of sodic and potassic chloride were introduced. In every experiment the thermometer was read at the same time with the rotation; and the average temperature, as well as its extreme variation, is given in the table. That the different experiments might be compared, we have expressed them by the equation—

$$y=a+bx+cx^2,$$

in which y is the angle of rotation, x the time in half-hours, counting from the first contact of the lactin with water, and a, b, and c, are constants. The values of a, b, and c, were calculated by the method of least squares.

In Table II are given the equations, accompanied by the probable error of a single comparison of the calculated and experimental values of y. The sum of the $\pm$ actual errors is in nearly all cases zero.

Table II.

Number.		Equation.		Probable error.
1	....	$y=13 \cdot 9002-\cdot 48543x+\cdot 014330x^2$	....	·0315
2	....	$y=14 \cdot 1325-\cdot 56919x+\cdot 017755x^2$	....	·0423
3	....	$y=13 \cdot 6284-\cdot 49476x+\cdot 014629x^2$	....	·0323
4	....	$y=15 \cdot 4100-\cdot 62775x+\cdot 021712x^2$	....	·0316
5	....	$y=14 \cdot 6188-\cdot 49366x+\cdot 013833x^2$	....	·0173
6	....	$y=15 \cdot 0692-\cdot 71727x+\cdot 026959x^2$	....	·0519
7	....	$y=15 \cdot 1537-\cdot 60585x+\cdot 019943x^2$	....	·0269
8	....	$y=15 \cdot 1654-\cdot 54298x+\cdot 016387x^2$	....	·0232
9	....	$y=15 \cdot 8792-\cdot 58006x+\cdot 017402x^2$	....	·0263
10	....	$y=14 \cdot 5430-\cdot 56770x+\cdot 018459x^2$	....	·0229
11	....	$y=14 \cdot 6154-\cdot 56240x+\cdot 018388x^2$	....	·0368
12	....	$y=15 \cdot 3747-\cdot 66860x+\cdot 023514x^2$	....	·0380
13	....	$y=18 \cdot 2142-\cdot 65254x+\cdot 020109x^2$	....	·0224
14	....	$y=16 \cdot 6262-\cdot 65155x+\cdot 020546x^2$	....	·0313
15	....	$y=17 \cdot 2230-\cdot 64521x+\cdot 020448x^2$	....	·0227
16	....	$y=14 \cdot 6339-\cdot 48232x+\cdot 013474x^2$	....	·0177
17	....	$y=15 \cdot 5954-\cdot 56252x+\cdot 017796x^2$	....	·0401
18	....	$y=16 \cdot 4546-\cdot 65417x+\cdot 022141x^2$	....	·0455
19	....	$y=17 \cdot 5923-\cdot 58714x+\cdot 016131x^2$	....	·0088
20	....	$y=14 \cdot 9011-\cdot 45421x+\cdot 012057x^2$	....	·0255

By the aid of these equations we can now calculate the initial specific rotation of lactin, or the rotation when $x=0$ calculated to unit of weight. When $x=0$, $y=a$; and the permanent rotation being known, the initial specific rotation $= \dfrac{a \times 59 \cdot 17}{\text{permanent rotation}}$. The following are the values found, the chloride experiments being averaged by themselves.

Average of Nos. 1, 2, 3, 10, 11, 12........	93°·98
„ „ Nos. 4, 5, 6	91·90
„ „ Nos. 7, 8, 9	91·97
„ „ Nos. 13, 14, 15	91·87
„ „ Nos. 16, 17, 18..............	91·37
Single experiment, No. 19	95·30
„ „ No. 20	92·16

Average of the twenty experiments, 92°·63.

On differentiating the equations, putting $\frac{dy}{dx}=0$, and calculating the values of x and y, we find that the values of y thus got do not correspond to the permanent rotation, but are always greater; showing that the change in rotatory power does not progress according to the

same law throughout, but that, at the point referred to, a new reaction begins. This value of y is proportional to the amount of lactin in solution, indicated by the permanent rotation; and the specific rotation calculated from it in the different experiments is practically constant, its average value (from twenty experiments) being 64°·8. The following are the values of x and y when $\frac{dy}{dx}=0$.

Table III.

No.	x.	y.	Specific rotation.
1	16·937	9°·789	66°·71
2	16·029	9·571	66·14
3	16·910	9·445	64·95
4	14·456	10·872	63·42
5	17·843	10·214	65·11
6	13·308	10·298	63·31
7	15·189	10·552	63·91
8	16·567	10·667	65·63
9	16·666	11·045	63·22
10	15·388	10·178	64·20
11	15·292	10·315	66·39
12	14·217	10·622	63·53
13	16·225	12·920	64·88
14	15·855	11·461	63·57
15	15·776	12·133	64·78
16	17·898	10·319	65·41
17	15·804	11·149	65·06
18	14·772	11·622	63·79
19	18·199	12·250	66·36
20	18·836	10·624	65·71

This break in the change seems to point to the dual nature of lactin mentioned by Fudakowski,* whose experiments show that lactin, like cannose, gives two glucoses—lacto glucose and galactose.

An increase of temperature evidently hastens the change; but the exact relation of temperature to the rate of change has not been discovered.

The presence of sodic or potassic chloride increases the amount of lactin in solution, but has no apparent effect on the rate of change.

IV. *Action of Hydric Nitrate on Lactin.*—We made an attempt to trace this action, but did not succeed in overcoming experimental difficulties. The first of these was the impossibility of completing the action with the quantity of acid required for the first change. If a

* "Deut. Chem. Ges. Ber.," ix, 42–44.

larger quantity of acid were used, the first changes were so rapid as to evade measurement; moreover, the oxalic acid formed, by crystallising in the acid liquid, made accurate observation impossible. By adding the acid in small successive portions, we nevertheless succeeded in obtaining an outline of the reaction, of which the curve drawn below is an accurate general expression.

Rotation.

Hydric Nitrate.

Dubrunfaut, who has also examined this action,* asserts that the rotatory power first rises to $\frac{13}{10}$ of the original amount, then falls gradually to zero, again rises to $\frac{1}{4}$ of the original rotation, and once more falls to zero: the highest rotation corresponding to galactose, the first point of inactivity to mucic acid; the second rise probably to dextro-tartaric acid; and the second fall to the formation of oxalic acid. Our experiments show the formation of a lævo-rotatory substance, perhaps lævo-tartaric acid. The general form of the curve constitutes it an interesting and novel addition to chemical curves.

V. *Note on Solubility.*—The mutual relations of water and lactin in solution undergo a change upon which the change of rotation most probably depends. Water shaken with a large quantity of very finely powdered lactin at a temperature of 17° C., takes up a quantity of lactin corresponding to a solubility of 1 part lactin in 10·64 parts water. With four hours' contact, the solubility increases to 1 part lactin in 7·49 parts water. The permanent solubility got by the analysis of the mother-liquor of lactin crystallized over oil of vitriol is 1 part lactin in 3·23 parts water. In the solution of the lactin a fall of temperature of 0°·45 C. was observed. Pohl† also found a depression of temperature (0°·88 C.); while Dubrunfaut alleges that heat is evolved.

Conclusions.

I. The initial specific rotation of lactin is 92°·63.

II. The permanent specific rotation of lactin is 59°·17.

* "*Compt.* rend.," xlii, 228. † "Journ. Pr. Chem.," lxxxii, 154.

III. The change of rotation of a solution of lactin can be expressed by a mathematical equation.

IV. When the specific rotation 64°·8 is reached, the law of change must be expressed by a different equation.

V. The initial solubility of lactin is 1 part lactin in 10·64 parts water.

VI. The permanent solubility is 1 part lactin in 3·23 parts water.

IV. "On the Microrheometer." By J. B. Hannay, F.R.S.E., F.C.S., lately Assistant Lecturer on Chemistry in the Owens College, Manchester. Communicated by H. E. Roscoe, LL.D., F.R.S., Professor of Chemistry in the Owens College, Manchester. Received December 11, 1878.

(Abstract.)

In this paper the author reviews the work done by chemists and physicists in determining the relation between the chemical composition of a liquid and its rate of flow through a capillary tube. Poiseuille* ascertained, in a very accurate manner, all the physical laws relating to the rate of flow, as regulated by temperature, pressure, and dimensions of the tube; but on examining saline solutions he could make nothing of the numbers presented, because he used percentage solutions instead of solutions proportional to the equivalent of the body dissolved. Graham,† noticing that Poiseuille had discovered a hydrate of alcohol by running various mixtures of alcohol and water through the tube, examined mixtures of the various acids with water, and found that the hydration proceeded by distinct steps of multiple proportions. Several others, notably Guerout,‡ have since worked on the same subject, but as they have only worked on organic liquids, and have done all the rates at the same temperature, the results throw no light on the phenomena. Thus water runs about five times as quickly at 100° as at 0°; and in a series of alcohols, such as Guerout experimented upon, the differences between their boiling points were very great, so that, their vapour tensions or molecular mobilities being quite incomparable while at the same temperature, the experiments do not admit of any real interpretation. The author reserves the organic part of the investigation, which requires the determination of vapour tensions, till a future paper, and in the present deals with saline solutions.

The phenomenon of the flow of liquids through capillary tubes has

* "Ann. de Chim. et de Physique," [3], t. vii, 50.

† "Phil. Trans.," 1861, p. 373.

‡ "Comptes rendus," lxxix, p. 1201; lxxxi, p. 1025.

been called in this country transpiration, while in other countries no distinct name has been adopted; and as the English word is already in use in French for another purpose, and properly applies to gases (the laws relating to which are quite different), the author proposes to use for liquids the term "Microrheosis," from μικρὸς and ῥέω, the instrument being called the microrheometer. The form of apparatus which the author finally adopted is figured in the paper, and is so arranged that when the liquid is introduced, as many experiments as may be desired may be tried, and the pressure and temperature, as well as the atmosphere in which the experiment is conducted, may be varied, while the thermometer indicating the temperature is at the mean point of the system. The author gives a curve for water from 0° to 100°, the differences of rate being smaller as the temperature rises.

Various salts are then examined, being dissolved to form "normal" solutions; but as the solubility of some salts is too low for such solutions, the effect of the amount of salts dissolved is determined. This is found to be directly proportional to the amount of salt in solution. Values for many salts in solution are then given, each number being the mean of ten experiments, and the probable error of the mean is calculated in each case. The conclusions arrived at are these. The rate of flow does not depend on any of the "mechanical" features of the salt, such as crystalline form, specific volume, solubility, &c.; but upon the mass of the elements forming the substance and the amount of energy expended in its formation. Each element has a value of its own, which is continued in all its compounds. Thus all the salts of potassium and sodium formed by the same acids have a constant difference. In like manner each metalloïd and acid radicle has a value which is continued in all its combinations. Then the greater the combining value of an element the quicker is its microrheosis; thus potassium has a higher rate than sodium, barium than strontium, strontium than calcium, and so on. The microrheosis also varies with the amount of energy in the compound; thus nitrates stand highest, as they contain most energy; then chlorides; and, lastly, sulphates, which are exhausted compounds.

The instrument, bringing to light as it does the fundamental relations of combining weight and energy in chemical action, will be of the utmost importance in chemical physics, as by its use, not only will the amount of energy evolved in reactions be determined, but the mass combined; or, in other words, the chemical equivalent of the elements involved will be found.

V. "Limestone as an Index of Geological Time." By T. MELLARD READE, C.E., F.G.S., F.R.I.B.A. Communicated by A. C. RAMSAY, D.C.L., F.R.S., Director-General of the Geological Survey of the United Kingdom. Received December 24, 1878.

(Abstract.)

The geological history of the globe is written only in its sedimentary strata, but if we trace its history backwards, unless we assume absolute uniformity, we arrive at a time when the first sediments resulted from the degradation of the original crust of the globe.

There is no known rock to which a geologist could point and say "that is the material from which all sedimentary rocks have been derived," but analogy leads us to suppose that if the earth had an igneous origin, the original materials upon which the elements first began to work were of the nature of granite or basalt.

From a variety of considerations drawn from borings, mines, faults, natural gorges and proved thicknesses of the strata of certain mountain chains, the author arrives at the conclusion that the sedimentary crust of the earth is at least of an average actual thickness of one mile, and infers from the proportionate amount of carbonates and sulphates of lime to materials in suspension in various river waters flowing from a variety of formations, that one-tenth of the thickness of this crust is calcareous.

Limestone rocks have been, geology tells us, in process of formation from the earliest known ages, but the extensive series of analyses of water made by Dr. Frankland for the Rivers Pollution Commission, shows that the later strata in Great Britain are much more calcareous than the earlier. The same holds true of the continent of Europe, and the balance of evidence seems in favour of the supposition that there has been on the whole a gradual progressive increase or evolution of lime. The "Challenger" soundings show that carbonate of lime in the form of tests of organisms is a general deposit characterising the greater part of the ocean bottoms, while the materials in suspension are, excepting in the case of transport by ice, deposited within a distance of 200 miles of land.

This wider distribution in *space* of lime, the author thinks, must also profoundly influence its distribution in *time*, and he shows this by example and illustration. It can also be proved to demonstration that the greater part of the ocean bottom must at one time or another

have been land, else the rocks of the continents would have become gradually less, instead of more, calcareous.

Thus the arguments drawn from the geographical distribution of animals are reinforced by physical considerations.

The author goes on to show that the area of granitic and volcanic rocks in Europe and the part of Asia between the Caspian and the Black Sea, as shown in Murchison's map of Europe, is two-twenty-fifths ($\frac{2}{25}$) of the whole; much of this is probably remelted sediments and some of the granites the product of metamorphism.

From considerations stated at length it is estimated that the area of exposures of igneous to sedimentary rocks would be for all geological time liberally averaged at one-tenth ($\frac{1}{10}$) of the whole.

These igneous rocks are either the original materials of the globe protruded upwards, or they are melted sediments or a mixture of the two.

The only igneous rocks we know of are of the nature of granites and traps. If these rocks do not constitute the substratum of the earth, and all known rocks, igneous as well as sedimentary, are derivative, either geological time is infinite, or the rock from which they are derived is, so far as we know, annihilated geologically speaking, and we have no records of it left.

If we assume the latter as true, the past is immeasurable, but in order to arrive at a minimum age of the earth, the author starts from the hypothesis that the fundamental rocks were granitic and trappean.

From eighteen analyses by Dr. Frankland, it is shown that the water flowing from granitic and igneous rock districts in Great Britain contains on an average 3·73 parts per 100,000 of sulphates and carbonates of lime.

The amount of water that runs off the ground is given for several of the great continental river basins in Europe, Asia, Africa, and America. The annual depth of rain running off the granitic and igneous rock areas, taking into consideration the greater height at which they usually lie and the possibility of greater rainfall in earlier ages, is averaged at 28 inches, and the annual contribution of lime in solution in the forms of carbonates and sulphates at 70 tons per square mile.

With these elements, and giving due weight to certain physical considerations that have been urged in limitation of the earth's age, the author proceeds to his calculations, arriving at this result, that the elimination of the calcareons matter contained in the sedimentary crust of the earth must have occupied at least 600 millions of years. The actual time occupied in the formation of the groups of strata as divided into relative ages by Professor Ramsay, is inferred as follows:—

	Millions of years.
Laurentian, Cambrian, and Silurian	200
Old Red, Carboniferous, Permian, and New Red	200
Jurassic, Wealden, Cretaceous, Eocene, Miocene, Pliocene, and Post-pliocene	200
	600

The concluding part of the paper consists of answers to objections. The author contends that the facts adduced prove geological time to be enormously in excess of the limits urged by some physicists, and ample to allow on the hypothesis of evolution for all the changes which have taken place in the organic world.

VI. "Preliminary Note on the Substances which produce the Chromospheric Lines." By J. NORMAN LOCKYER, F.R.S. Received December 24, 1878.

Hitherto, when observations have been made of the lines visible in the sun's chromosphere, by means of the method introduced by Janssen and myself in 1868, the idea has been that we witness in solar storms the ejection of vapours of metallic elements with which we are familiar from the photosphere.

A preliminary discussion of the vast store of observations recorded by the Italian astronomers (chief among them Professor Tacchini), Professor Young, and myself, has shown me that this view is in all probability unsound. The lines observed are in almost all cases what I have elsewhere termed and described as *basic lines;* of these I only need for the present refer to the following:—

b_3 ascribed by Ångström and Kirchoff to iron and nickel.
b_4 ,, Ångström to magnesium and iron.
5268 by Ångström to cobalt and iron.
5269 ,, ,, calcium and iron.
5235 ,, ,, cobalt and iron.
5017 ,, ,, nickel.
4215 ,, ,, calcium, but to strontium by myself.
5416 an unnamed line.

Hence, following out the reasoning employed in my previous paper, the bright lines in the solar chromosphere are chiefly lines due to the not yet isolated bases of the so-called elements, and the solar phenomena in their totality are in all probability due to dissociation at the photospheric level, and association at higher levels. In

this way the vertical currents in the solar atmosphere, both ascending and descending, intense absorption in sun-spots, their association with the faculæ, and the apparently continuous spectrum of the corona and its structure, find an easy solution.

We are yet as far as ever from a demonstration of the cause of the variation in the temperature of the sun; but the excess of so-called calcium with minimum sun-spots, and excess of so-called hydrogen with maximum sun-spots follow naturally from the hypothesis, and afford indications that the temperature of the hottest region in the sun closely approximates to that of the reversing layer in stars of the type of Sirius and *α* Lyræ.

If it be conceded that the existence of these lines in the chromosphere indicates the existence of basic molecules in the sun, it follows that as these lines are also seen generally in the spectra of two different metals in the electric arc, we must be dealing with the bases in the arc also.

January 30, 1879.

W. SPOTTISWOODE, M.A., D.C.L., President, in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "On the Effect of Heat on the Di-iodide of Mercury, HgI_2." By G. F. RODWELL, Science Master, and H. M. ELDER, a Pupil, in Marlborough College. Communicated by Professor TYNDALL, F.R.S., Professor of Natural Philosophy in the Royal Institution. Received January 9, 1879.

In continuation of the experiments on the effects of heat on the chloride, bromide, and iodide of silver, which one of us has previously had the honour of communicating to the Society,* it was thought to be advisable to search in some of the other metallic iodides for molecular anomalies similar to those presented by the iodide of silver. Among these no substance appeared more likely to possess such anomalies than the di-iodide of mercury. This substance, as is well known, is dimorphous. In the amorphous condition it presents the appearance of a brilliant scarlet powder, which, if heated, fuses at 200° C., and volatilises just above the fusing point to a vapour more

* "Proc. Roy. Soc.," vol. xxv, p. 280.

than twice as dense as that of mercury. The vapour condenses to rhombic prismatic crystals, which frequently become scarlet while cooling, but which, if they still remain yellow when cold, instantly become scarlet if rubbed or otherwise mechanically agitated. According to Warington this change is due to the transformation of the rhombic prisms into acute square-based octohedrons with truncated summits. If the yellow prismatic crystals are placed under the microscope, and are then touched, the change to the red variety may be observed to go on through the mass of contiguous crystals, accompanied by a slight movement, but the external form of the crystals remains unchanged, consequently pseudomorphous crystals are produced, and the larger rhombic prisms have been resolved into a mass of minute octohedrons. Frankenheim asserts that by the application of a very gentle heat, both the red and the yellow crystals may be sublimed together, and he believes that the vapour of the yellow crystals passes off at a lower temperature than that of the red. Warington found that the precipitate produced by iodide of potassium in chloride of mercury, appeared under the microscope to be composed of rhombic laminæ, which gradually altered their form by the truncation of the edges, until they disappeared, while square-based octohedrons were produced in their place.

The iodide is clearly capable of existing in two crystalline forms belonging to different systems, and of passing from the one form to the other, either by diminution of temperature or by simple mechanical means. Such a substance would seem to be likely to possess peculiarities in its modes of expansion under the influence of heat. In order to test this the iodide was submitted to the same experimental treatment as that employed in the case of the iodide of silver, and previously described in detail.

Homogeneous rods of the iodide of mercury were heated in paraffine in the expansion apparatus described and figured in the previous paper, and the extent of expansion due to a given range of temperature was noted. The apparatus was standardised by means of a rod of fine homogeneous silver. The same micrometer, reading to $\frac{1}{50000}$th of an inch, was employed, and the mode of conducting the experiments was precisely the same as in the case of the iodide of silver. Two slight changes were made in the apparatus however:—the one consisted in the substitution of a massive stone base for the wooden one hitherto used; and the other the replacement of the glass rods moving in stuffing boxes, by curved equal-armed levers moving over the rim of the trough, by which means the leakage of hot paraffine at the stuffing boxes was prevented.

Bars of the iodide of mercury were cast in clean glass tubes, and here at the outset the experimental difficulties commenced. For not only was it difficult to obtain a homogeneous rod, on account of the

volatilisation of the iodide at a temperature slightly exceeding its melting point, but the rod when cold was found to be so brittle that it usually broke in the attempt to remove the glass envelope from the outside. Eventually good rods were procured by slowly melting the iodide in thin glass tubes and annealing in hot paraffine. When the whole was cold the glass was cut on the outside, and carefully broken off the ends of the rod, which were sawed plane by a fine steel saw, and then furnished with metal caps, and the rod was placed between the levers of the expansion apparatus. After heating the bar once or twice in paraffine to a temperature approaching its melting point, longitudinal rifts appeared in the glass envelope, which was then easily removed, leaving a clean homogeneous rod of the iodide.

On heating a mass of the crimson amorphous iodide, it turns yellow at 126° C., and just before the melting point is attained the yellow changes to a deep red-brown. The liquid resulting from the fusion has the appearance of liquid iodide of silver, that is to say, it has the exact colour of bromine. The liquid when cooled solidifies to a red-brown solid which speedily becomes yellow, and at 126° C. it changes to the crimson octohedral variety. Distinct cracking sounds, due to inter-molecular movements, were heard during the continuance of the change. Heat is absorbed when the red iodide changes to yellow, and is given out when the yellow iodide changes to the red.

A bar of the iodide was placed in the expansion apparatus, melted paraffine was poured upon it, and when the index had become quite steady, a gentle heat was applied to the paraffine. The index showed a regular and slow expansion until a temperature of 126° C. was reached, when the bar began to change from the octohedral to the prismatic condition, and without further rise of temperature rapid expansion took place. The temperature was kept constant until the change was complete, and was then slowly raised. A regular expansion now took place under a higher coefficient than before the molecular change, and this continued until the melting point was attained. The results were concordant.

The expansion in passing from the solid to the liquid condition was determined by weighing mercury in a tube, and afterwards filling it to the same height with fused iodide. The specific gravity of each substance being known, and the weight of equal volumes, the expansion could obviously be readily determined.

The coefficient of cubical expansion for 1° C. from 0° C. to the point of change—126° C.—was found to be:—

·0000344706.

At 126° C., during the change from the red octohedral to the yellow prismatic condition, the body increased in bulk to the extent of:—

·00720407.

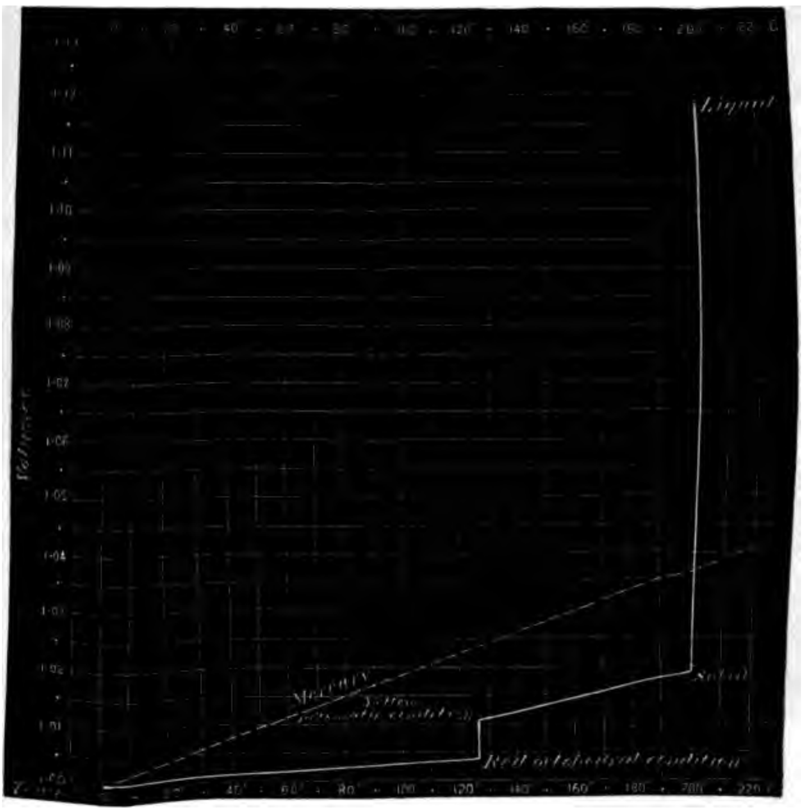
The coefficient of cubical expansion for 1° C. from 126° C., after the change to the melting point 200° C., was

·0001002953.

Thus, if we suppose a molten mass of the iodide of mercury to be cooling down from 200° C. to 0° C., the following would be the volumes under the conditions indicated:—

Volume at 200° C. of the liquid mass	= 1·1191147
,, ,, ,, solid mass	= 1·0190453
,, 126° C. (yellow prismatic condition)	= 1·0115378
,, ,, (red octohedral condition)	= 1·0043337
,, 0° C.	= 1·0000000

The changes are shown at one view in the accompanying curve table, in which the expansion of mercury is given for comparison.



According to Schiff the specific gravity of the octohedral iodide is 5·91; while Karsten makes it 6·2009, and Boullay 6·320.

Two distinct specimens with which we worked gave respectively—

(1). 6·3004
(2). 6·2941

The specific gravity of the fused iodide was found by the method before described to be

5·2865.

Thus the specific gravities corresponding to the five marked conditions shown in the curve table are as follows:—

Specific gravity at	0° C.	....	=	6·297
,, ,,	126° C.	(octohedral condition)	=	6·276
,, ,,	,,	(prismatic condition)	=	6·225
,, ,,	200° C.	solid	=	6·179
,, ,,	,,	liquid	=	5·286

II. "A Comparison of the Variations of the Diurnal Range of Magnetic Declination as recorded at the Observatories of Kew and Trevandrum." By BALFOUR STEWART, F.R.S., Professor of Natural Philosophy in Owens College, Manchester, and MORISABRO HIRAOKA. Received January 10, 1879.

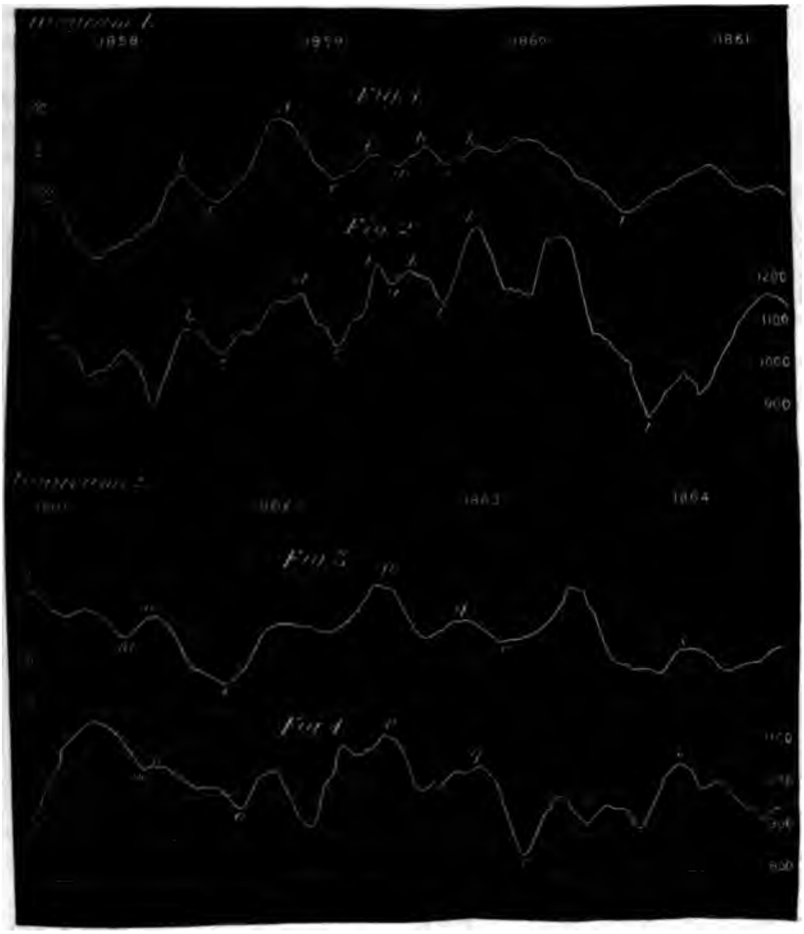
In a previous paper by one of the authors ("Proc. Roy. Soc.," vol. xxvi, p. 102) a table is given (Table II) exhibiting monthly means of the Kew diurnal declination-range, corresponding to forty-eight points in each year, or four for each month, that is to say, approximately one every week; and, in another paper ("Proc. Roy. Soc.," vol. xxvii, p. 81), another similar table exhibits monthly means of the Trevandrum diurnal declination-range for weekly points. In the present paper these two tables are compared together.

It became obvious to the writers, when engaged in making this comparison, that the turning points in the curve, which represented the variations of the Kew declination-range, were on the whole in point of time before the corresponding points in the Trevandrum curve.

While this result might have been rendered evident by making the numbers of the tables above-mentioned at once into curves, yet it was found to become more apparent to the eye and freer from inequalities by adopting a certain amount of equalization.

Accordingly, the Kew and Trevandrum tables were transformed into others, with the same time-interval between their numbers as in

the originals; but each number in the transformed table being the mean of nine consecutive numbers in the original table. Curves were then plotted from these transformed tables. In the diagrams attached to this paper, these equalized curves are compared together for the two observatories, figs. 1 and 3 giving the Kew curves, and figs. 2 and 4 those for Trevandrum. Points in the two curves, which are supposed to correspond, are represented by similar letters of the alphabet.



A comparison of these curves appears to lead to the following conclusions:—

(1.) Generally speaking, maximum points or risings in the one curve must be associated with maximum points or risings in the

other, rather than with minimum points or depressions. Indeed, the researches of Broun and others, from a different point of view, strengthen this conclusion, which is, however, abundantly supported by a glance at the curves themselves;

(2.) The oscillations of the Trevandrum curve are greater than those of the Kew curve;

(3.) In many cases where there is a want of striking likeness between the oscillations of the two curves, there are yet noticeable traces in the one curve corresponding to the oscillations of the other. There are, however, a few cases where there is a want of apparent likeness.

(4.) In general, though not invariably, the oscillations of the Trevandrum curve follow rather than precede the corresponding oscillations of the Kew curve. This will be perceived from the following numerical estimate:—

TABLE I.—Exhibiting the lagging behind of the Trevandrum Curve in Point of Time.

	Oscillations.	Trevandrum minus Kew in days.
1858	*a*	+15
	b	+30
1859	*c*	+11
	d	+32
	e	+ 8
	f	+11
	g	−13
	h	−30
1860	*i*	−11
	k	− 8
	l	+40
1861	*m*	+19
	n	0
1862	*o*	+25
1863	*p*	+11
	q	+21
	r	+25
1864	*s*	−11
	Mean	+ 9·7 days.

We venture to present the evidence in its present form, but forbear, in the meantime, to discuss the subject at greater length.

III. "On the Determination of the Rate of Vibration of Tuning Forks." By HERBERT MCLEOD, F.C.S., and GEORGE SYDENHAM CLARKE, Lieut. R.E. Communicated by Lord RAYLEIGH, F.R.S. Received January 16, 1879.

(Abstract.)

The paper contains a description of some experiments made with a view to determine the absolute pitch of tuning forks by means of a method proposed by the writers in a previous paper ("Proc. Roy. Soc.," vol. xxvi, p. 162).

It commences with a description of the time measurer adopted, consisting of a compensated pendulum, worked by electricity, the impulse being given by a driver depending for its action on gravity alone. The pendulum is arranged to give second contacts, driving a clock wheel with sixty teeth. This wheel has a platinum pin giving minute contacts, but it is used merely as a switch, the circuit being closed by the pendulum itself. The current works a relay, and closes the circuit required.

The tuning fork apparatus consists of a brass drum resting on friction wheels, and driven by a weight and train. Uniformity of motion being of great importance, an air-regulator, consisting of a fan enclosed in the lower compartment of a cylindrical box, is employed. By means of a diaphragm and vanes the fan can be made to do more or less work by pumping air from the lower into the upper compartment. The fan spindle carries a pulley driven by a thread passing round the drum.

Round one end of the drum are wrapped strips of paper on which white equidistant lines have been so ruled that they are parallel to the axis of the drum when the strips are in position. The strip most frequently used has 486 lines round the complete circumference of the drum. Opposite this graduated strip is placed a microscope with its axis horizontal. In the substage is placed a 2″ objective, producing an image of the graduations at the focus of the object-glass of the instrument. At the common focus of the two lenses is placed the tuning fork, the stem of which is held vertical in a vice. The fork is partially enclosed in a glass case, and is so adjusted that the image of one of its limbs seems to cut the image of the graduations at right angles. The fork is set in motion by a suspended double-bass bow. If when the fork is in vibration the drum is made to rotate with such a velocity that one of the graduations passes over the interval between two adjacent graduations in the time of one vibration of the fork, a stationary wave is seen of length equal to the length of that interval. To determine the number of vibrations of the fork in a given time, it

is only necessary, therefore, to be able to count the number of graduations which pass in that period. As a perfectly uniform rotation has not been obtained, a regulator under the control of the operator is employed. This consists merely of a piece of string which passes round the axis of the drum, and also round a pulley which can be turned by the operator's left hand. An upward or downward motion of the wave denotes that the drum is going too fast or too slow, and by means of the pulley a gentle check or acceleration sufficient to keep the wave steady is given to the drum.

An electric counter gives the number of complete revolutions accomplished by the drum in any given period, and a fine-pointed tube, containing magenta, is carried by a saddle above the drum, and being actuated by an electro-magnet, makes a dot on a piece of white paper wrapped round the drum at the beginning and end of the experiment. The distance apart of these dots gives the additional fraction of a revolution accomplished by the drum during the period of the experiment. Electric circuits are so arranged that a reverser turned a few seconds before the minute at which it is intended to begin the experiment, causes a current to be sent exactly at that minute by the clock relay, which starts the electric counter, and also makes a dot on the drum. Just before the expiration of the last minute of the experiment, the reverser is turned in the opposite direction, and at the expiration of that minute the counter is stopped, and a second mark made on the drum.

Some of the results obtained with different forks are given.

The results of further experiments made to determine the effect of temperature, of continuous and intermittent bowing, and of the mode of fixing the fork, are appended.

An optical method by which two slightly dissonant forks may be compared without altering the period of either, is described.

Figures and diagrams fully explaining the apparatus employed, accompany the paper.

IV. "On certain means of Measuring and Regulating Electric Currents." By C. William Siemens, D.C.L., F.R.S. Received January 16, 1879.

[Plates 4, 5.]

The dynamo-electric machine furnishes us with a means of producing electric currents of great magnitude, and it has become a matter of importance to measure and regulate the proportionate amount of current that shall be permitted to flow through any branch circuit, especially in such applications as the distribution of light and mechanical force.

On the 19th of June last, upon the occasion of the Soirée of the President of the Royal Society, I exhibited a first conception of an arrangement for regulating such currents, which I have since worked out into a practical form. At the same time, I have been able to realize a method by which currents passing through a circuit, or branch circuit, are measured, and graphically recorded.

It is well known that when an electric current passes through a conductor, heat is generated, which, according to Joule, is proportionate in amount to the resistance of the conductor, and to the square of the current which passes through it in a unit of time, or

$$H = C^2R.$$

I propose to take advantage of this well-established law of electro-dynamics, in order to limit and determine the amount of current passing through a circuit, and the apparatus I employ for this purpose is represented on figs. 1 to 3, Plate 4, of the accompanying drawings. Letters of reference to the principal parts of the instrument are given on the foot-note of the drawing.

The most essential part of the instrument is a strip (A) of copper, iron, or other metal, rolled extremely thin, through which the current to be regulated has to pass. One end of this thin strip of metal is attached to a screw (B), by which its tension can be regulated; it then passes upwards over an elevated insulated pulley (I), and down again to the end of a short lever, working on an axis, armed with a counter-weight and with a lever (L), whose angular position will be materially affected by any small elongation of the strip that may take place from any cause. The apparatus further consists of a number of prisms of metal (P), supported by means of metallic springs (M), so regulated by movable weights (W) as to insure the equidistant position of each prism from its neighbour, unless pressed against the neighbouring piece by the action of the lever (L), in consequence of a shortening of the metallic strip. By this action, one prism after another would be brought into contact with its neighbour, until the last prism in the series would be pressed against the contact spring (S), which is in metallic connexion with the terminal (T).

The current passing through the thin strip of metal will, under these circumstances, pass through the lever (L) and the line of prisms to the terminal (T), without encountering any sensible resistance. A second and more circuitous route is, however, provided between the lever (L) and the terminal (T), consisting of a series of comparatively thin coils of wire of German silver or other resisting metal (R, R), connecting the alternate ends of each two adjoining springs, the first and last spring being also connected to the lever (L) and terminal (T) respectively.

When the lever (L) stands in its one extreme position, as shown in

the drawing, the contact pieces are all separate, and the current has to pass through the entire series of coils, which present sufficient aggregate resistance to prevent the current from exceeding the desired limit.

When the minimum current is passing, the thin metallic strip is at its minimum working temperature, and all the metallic prisms are in contact, this being the position of least resistance. As soon as the current passing through the apparatus shall increase in amount, the thin metallic strip will immediately rise in temperature, which will cause it to elongate, and will allow the lever (L) to recede from its extreme position, liberating one contact piece after another. Each such liberation will call into action the resistance coil connecting the spring ends, and an immediate corresponding diminution of the current through increased resistance; additional resistance will thus be thrown into the circuit, until an equilibrium is established between the heating effect produced by the current in the sensitive strip, and the diminution of heat by radiation from the strip to surrounding objects. In order to obtain uniform results, it is clearly necessary that the loss of heat by radiation should be made independent of accidental causes, such as currents of air or rapid variations of the external temperature, for which purpose the strip is put under a glass shade, and the instrument itself should be placed in a room where a tolerably uniform temperature of say 15° C. is maintained. Under these circumstances, the rate of dissipation by radiation and conduction (considering that we have to deal with low degrees of heat) increases in arithmetical ratio with the temperature of the strip; the expansion of the strip, which affects the position of the lever (L), is proportionate to the temperature which is itself proportionate to the square of the current—a circumstance highly favourable to the sensitive action of the instrument.

Suppose that the current intended to be passed through the instrument is capable of maintaining the sensitive strip at a temperature of say 60° C., and that a sudden increase of current takes place in consequence either of an augmentation of the supply of electricity or of a change in the extraneous resistance to be overcome, the result will be an augmentation of temperature, which will continue until a new equilibrium between the heat supplied and that lost by radiation is effected. If the strip is made of metal of high conductivity, such as copper or silver, and is rolled down to a thickness not exceeding 0·05 milim., its capacity for heat is exceedingly small, and its surface being relatively very great, the new equilibrium between the supply of heat and its loss by radiation is effected almost instantaneously. But, with the increase of temperature, the position of the regulating lever (L) is simultaneously affected, causing one or more contacts to be liberated, and as many additional resistance coils to be thrown

into circuit: the result being that the temperature of the strip varies only between very narrow limits, and that the current itself is rendered very uniform, notwithstanding considerable variation in its force, or in the resistance of the lamp, or other extraneous resistance which it is intended to regulate.

It might appear at first sight that, in dealing with powerful currents, the breaking of contacts would cause serious inconvenience in consequence of the discharge of extra current between the points of contact. But no such discharges of any importance actually take place, because the metallic continuity of the circuit is never broken, and each contact serves only to diminish to some extent the resistance of the regulating rheostat. The resistance coils, by which adjoining contact springs are connected, may be readily changed, so as to suit particular cases; they are made by preference of naked wire, in order to expose the entire surface to the cooling action of the atmosphere.

In dealing with feeble currents, I use another form of regulator, in which disks of carbon are substituted for the wire rheostat. The Count du Moncel, in 1856, first called attention to, and Mr. Edison more recently took advantage of, the interesting circumstance that the electrical resistance of carbon varies inversely with the pressure to which it is subjected, and by piling several disks of carbon one upon another in a vertical glass tube, a rheostat may be constructed which varies between wide limits, according as the mechanical pressure in the line of the axis is increased or diminished. Fig. 4, Plate 5, represents the current regulator based upon this principle, and the foot-notes below the figure furnish the explanation of parts. A steel wire of say 0·3 milim. diameter is drawn tight between the end of a bell-crank lever (L) and an adjusting screw (B), the pressure of the lever being resisted by a pile of carbon disks (C) placed in a vertical glass tube. The current passing through the steel wire, through the bell-crank lever, and through the carbon disks, encounters the minimum resistance in the latter so long as the tension of the wire is at its maximum; whereas the least increase in temperature of the steel wire by the passage of the current causes a decrease of pressure upon the pile of carbon disks, and an increase in their electrical resistance; it will thus be readily seen that, by means of this simple apparatus, the strength of small currents may be regulated so as to vary only within certain narrow limits.

The apparatus described in figs. 1 to 3, Plate 4, may be adapted also for the *measurement* of powerful electric currents—an application which is represented by figs. 5 and 6, Plate 5. The variable rheostat is in this case dispensed with, and the lever (L) carries at its end a pencil (P) pressing with its point upon a strip of paper drawn under it in a parallel direction with the lever by means of clockwork. A second fixed pencil (D) draws a second or datum line upon the strip, so

adjusted that the lines drawn by the two pencils coincide when no current is passing through the sensitive strip. The passage of a current through the strip immediately causes the pencil attached to the lever to move away from the datum line, and the distance between the two lines represents the temperature of the strip. This temperature depends, in the first place, upon the amount of current passing through the strip, and, in the second place, upon the loss of heat by radiation from the strip; which two quantities balance one another during any interval that the current remains constant.

If C is the current before increase of temperature has taken place;

R the resistance of the conductor at the external temperature (T);

H the heat generated per unit of time at the commencement of the flow;

R′ the resistance, and H′ the heat, when the temperature T′ and the current C′ have been attained;

Then by the law of Joule—

$$H'=R'C'^2.$$

But inasmuch as the radiation during the interval of constant current and temperature is equal to the supply of heat during the same interval, we have by the law of Dulong and Petit—

$$H'=(T'-T)S,$$

in which S is the radiating surface. Then

$$R'C'^2=(T'-T)S$$
$$C'^2=(T'-T)\frac{S}{R'}.$$

But T′ − T represents the expansion of the strip, or movement of the pencil m, and considering that the electrical resistance of the conductor varies as its absolute temperature (which upon the Centigrade scale is 274° below the zero Centigrade) according to a law first expressed by Helmholtz, and that we are only here dealing with a few degrees difference of temperature, no sensible error will be committed in putting the value of R for R′, and we have the condition of equilibrium

$$C'^2=m\frac{S}{R}\ \therefore\ C'=\sqrt{m\frac{S}{R}},\quad (1)$$

or, in words, the current varies as the square root of the difference of temperature or ordinates.

For any other condition of temperature T″ we have

$$C''^2=\frac{S}{R}(T''-T)$$
$$\therefore\quad C''=\sqrt{\frac{S}{R}(T''-T)}.$$

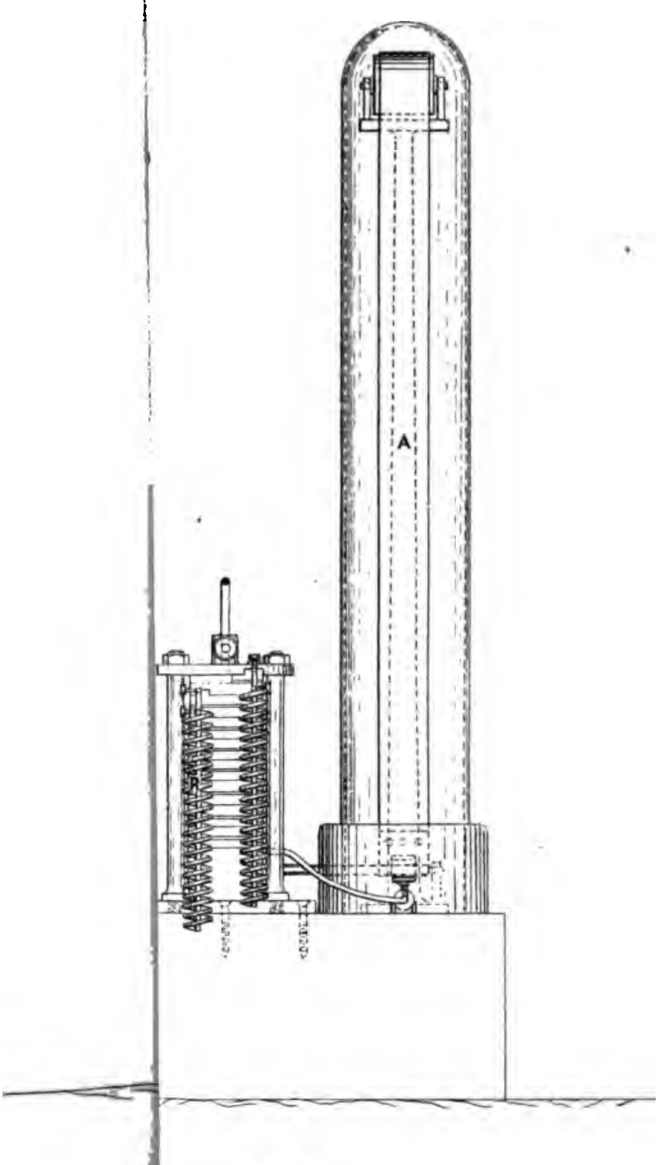


Fig. 2.

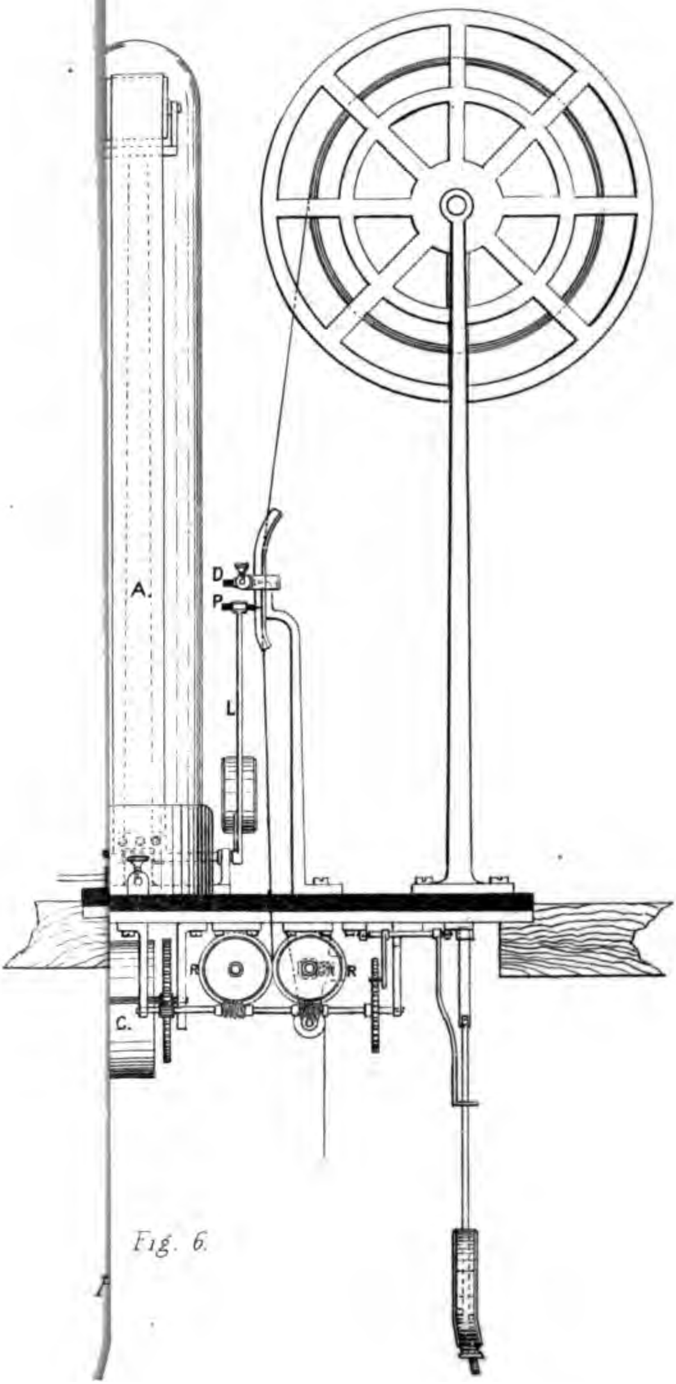


Fig. 6.

and $(C''^2 - C'^2) = (T'' - T - T' + T)\frac{S}{R} = (T'' - T')\frac{S}{R}$,

but for small differences of C'' and C' we may put

$$(C''^2 - C'^2) = 2C''(C'' - C'),$$

that is to say, small variations of current will be proportional to the variation in the temperature of the strip.

In order to facilitate the process of determining the value of a diagram in webers or other units of current, it is only necessary, if the variations are not excessive, to average the ordinates, and to determine their value by equation (1), or from a table prepared for that purpose. The error committed in taking the average ordinate instead of the absolute ordinates, when the current varies between small limits, is evidently small, the variation of the ordinates above their mean value averaging the variations below the same.

The thin sensitive conductor may thus be utilized either to restrict the amount of electricity flowing through a branch circuit within certain narrow limits, or to produce a record of the amount of current passed through a circuit in any given time.

Presents, January 9, 1879.

Transactions.

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February 6, 1879.

W. SPOTTISWOODE, M.A., D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On certain Dimensional Properties of Matter in the Gaseous State. Part I. Experimental Researches on Thermal Transpiration of Gases through Porous Plates, and on the Laws of Transpiration and Impulsion, including an Experimental Proof that Gas is not a Continuous Plenum. Part II. On an Extension of the Dynamical Theory of Gas which includes the Stresses, Tangential and Normal, caused by a Varying Condition of the Gas, and affords an explanation of the Phenomena of Transpiration and Impulsion." By OSBORNE REYNOLDS, F.R.S., Professor of Engineering at Owens College. Received January 17, 1879.

Abstract of Part I (Experimental).

Section I (Introduction).

1. The motion of gases through minute channels such as capillary tubes, porous plugs, and apertures in thin plates has been the subject of much attention during the last fifty years. The experimental study of these motions, principally by Graham, resulted in the discovery of important properties of gases, and it is largely, if not mainly, as affording an explanation of these properties, that the molecular theory has obtained such general credence.

It does not appear, however, that either the experimental investigations of these motions, or the theoretical explanations of the properties revealed, have hitherto been in any sense complete. There exists a whole class of very marked phenomena which have escaped the notice of Graham and other observers, while several of the most marked and important facts discovered by Graham have hitherto remained unconnected by any theory.

2. Amongst the best known of the phenomena is the difference in the rates at which different gases transpire through minute channels, and the consequent difference in the pressure which ensues when two different gases, initially at the same pressure, are separated by a porous plate. But it does not appear that hitherto any attempt has been made to ascertain the existence of what may be considered a closely analogous phenomenon—that a difference of temperature on the two sides of the plate might cause gas, without any initial difference of pressure or any difference in chemical constitution, to pass through the plate—nor am I aware that such a result from a difference of temperature has been in any way surmised.

I have now ascertained by experiments, which will be described at length, that a difference of temperature may be a very potent *cause of transpiration* through porous plates. So much so, that

with hydrogen on both sides of a porous plate, the pressure on the one side being that of the atmosphere, a difference of 160° F. (from 52° to 212°) in the temperature on the two sides of the plate secured a permanent difference in the pressure on the two sides equal to an inch of mercury; the higher pressure being on the hotter side. With different gases and different plates various results were obtained, which, however, as will be seen, are connected by definite laws.

3. Again, although Graham found that he obtained not only very different results, but also very different laws of motion with plates of different coarseness, or with plates and capillary tubes, neither he nor any subsequent observer appears to have followed up this lead. As regards Graham, this appears to me to be somewhat surprising. For although he may have considered the mere difference in the results to have been analogous to the difference found by Poiseuille for liquids, it would seem as though the difference in the laws of motion should have excited his curiosity; and then, as he was avowedly of opinion that gas is molecular, he could hardly have failed to observe that so long as the mean distance separating the molecules in the gas bore a fixed relation to the breadth of the openings in his plates, he should have had the same laws of motion. This view, however, appears to have escaped him as well as all subsequent observers, otherwise it would have been seen that with a simple gas such as hydrogen, similar results must be obtained so long as the density of the gas is inversely proportional to the lateral dimensions of the passages through the plates.

By experiments to be described I have now fully established this law. I find that with different plates similar results are obtained when the densities of the gas with the different plates bear certain fixed ratios, and that this is the case whatever may be the cause of transpiration, *i.e.*, a difference of temperature or a difference of pressure; a difference of gas I have not investigated, as it was obviously unnecessary to do so. Thus, with two plates, one of stucco and the other of meerschaum, similar results of transpiration caused by pressure were obtained when the densities with the two plates were respectively as 1 to 5·6 both with hydrogen and air, and at pressures ranging from 30 to 2 inches of mercury; also with the same two plates similar results of thermal transpiration were obtained when the densities were respectively as 1 to 6·5 both for hydrogen and air, and through a range of densities from 30 inches to ·25 of an inch of mercury, the discrepancy between 5·6 and 6·5 being in all probability owing to a slightly altered condition of the plates.

This correspondence of the results at corresponding densities holds although the law of motion changes. Thus, with air at 30 inches through the stucco plate, the law of motion was the same as that found by Graham for a stucco plate, while at the smallest pressure

(·25 inch) it was nearly the same as he found for graphite plates, or for apertures in thin plates.

Having established this law of corresponding results at corresponding densities, it became apparent that the results obtained with plates of different coarseness, and with the same plates, but with different densities of gas, followed a definite law. This law, which admits of symbolical expression, shows that there exists a definite relation between the results obtained, the lateral dimensions of the passages, and the density of the gas.

This law is important in reconciling results which have hitherto appeared to be discordant and as tending to complete the experimental investigation, but it has another and a more general importance.

It may not appear at first sight, but on consideration it will be seen that this law amounts to nothing less than an absolute experimental demonstration that gas possesses a heterogeneous structure—that it is not a continuous plenum of which each part into which it may be divided has the same properties as the whole.

It would appear that Graham must have had this proof, so to speak, under his eyes, and it is strange that both he and other observers have overlooked it. It seems possible, however, that they were not alive to the importance of such a demonstration. It is now so generally assumed that gas is molecular, that the weakness of the evidence on which the assumption is based and the importance of further proof are points which are apt to escape notice.

The Importance of an Experimental Demonstration that Gas Possesses Molecular Structure.

5. The idea of molecular gas does not appear to have originated from the recognition of properties in gas which were inconsistent with the idea of a continuous plenum, but from a wish to reconcile the properties of gas with the properties of other substances, or, more strictly, with some general property of matter. And the general conviction which may be said to prevail at the present time is owing to the simplicity of the assumptions on which the molecular hypothesis is based, and the completeness with which many of the properties of gas have been shown to result from this hypothesis. But it will be readily seen that however simple may be the assumptions of the kinetic theory, and however completely the properties of gases may be shown to follow from these assumptions, this is no disproof of the possibility that gas may be a continuous substance, each elementary portion of which is endowed with all the properties of the whole, and unless this is disproved there may exist doubts as to the necessity for the kinetic theory.

Any direct proof, therefore, that gas is not ultimately continuous altogether alters the position of the molecular hypothesis.

The Sufficiency of the Demonstration that Gas is not Structureless.

6. In order to prove that gas is not structureless, it is not necessary that we should be able to perceive the actual structure; we have only to find some property of a certain quantity of gas which can be shown not to be possessed by all the parts, some property which is altered by a rearrangement of the parts.

Hitherto I believe that no such property has been recognised, or, at all events, the conclusions to be drawn from such a property have not been recognised. The phenomena of transpiration, as well as those of the radiometer, depend on such properties, but these properties have not been sufficiently understood to bring out the conclusion. This conclusion, however, follows directly from the law indicated in Article 4, viz., that the results of transpiration depend on the relation between the size of the passages and the density of the gas.

The Results Deduced from Theory.

7. Although the existence of the phenomena of thermal transpiration, and the existence of the law of corresponding results at corresponding densities, have been verified by experiment, they were not so discovered; they followed from what appeared to be a successful attempt to complete the explanation which I had previously offered of the forces which result when heat is communicated from a surface to a gas,* and the phenomena of the radiometer.

Having found, what I had not at first perceived, that according to the kinetic theory the excess of pressure resulting from the communication of heat to a gas must depend on the fact of the surface from which the heat flows being of limited extent, and must follow a law depending on some relation between the mean path of a molecule and the size of the surface, it appeared that by using vanes of comparatively small size the force should be perceived at correspondingly greater pressures of gas.

On considering how this might be experimentally tested, it appeared that to obtain any result at measurable pressures the vanes would have to be very small indeed; too small almost to admit of experiment. And it was while searching for some means to obviate this difficulty that I came to perceive that if the vanes were fixed, then instead of the movement of the vanes we should have the gas moving past the vanes—a sort of inverse phenomenon; and then instead of small vanes small spaces might be allowed for the gas to pass. Whence it was at once obvious that in the porous plugs I should have the means of verifying these conclusions. I followed up the idea, and by a method which I devised of extending the dynamical theory of gases, so as to take into account the forces (tangential and normal) arising from a

* "Proc. Roy. Soc.," vol. xxii, p. 402, and "Phil. Trans.," vol. clxvi, p. 725.

varying condition of molecular gas, I was able to deduce what appears to me to be a complete theory of transpiration.

This theory appears to include all the results established by Graham, as well as the known phenomena of the radiometer, which, for the sake of shortness, I shall call the phenomena of *Impulsion*. I was also able definitely to deduce the results to be expected as regards both thermal transpiration and the law of corresponding densities for transpiration and impulsion.

Having made these deductions I commenced the experiments on transpiration, which so completely verified my theoretical deductions that I have been able to produce the theory in its original form, with some additions but without any important modification.

Moreover, having succeeded (not without some trouble) in rendering apparent the effect of differences of temperature in causing gas to move through fine apertures, I recurred to the original problem, and by suspending fibres of silk and spider lines to act as vanes, I have now succeeded in directly verifying the conclusion that the pressure of gas at which the force in the radiometer becomes apparent varies inversely as the size of the vanes. With the fibre of silk I obtained repulsion at pressures of half an atmosphere.

The Arrangement of the Paper.

8. My object in this paper is to describe the reasoning by which I was led to undertake the experiments, as well as the experiments themselves; but as the theory will be better understood after an acquaintance with the facts, I have inverted the natural order and given the experiments first. I include here, however, a somewhat full account of the results to be expected as deduced from the theory.

Then follows a statement of the laws of transpiration and impulsion as deduced from theory:—

Section II is devoted to the description of the experiments on thermal transpiration; Section III to the experiments on transpiration under pressure, and Section IV to the experiments on impulsion.

In this abstract it will not be possible to give more than a sketch of the matter contained in these sections. The numerous precautions and tests will have to be left unnoticed, and only a few of the experimental results can be given. The investigation occupied from February, 1878, till the beginning of August, every result being verified by repeated experiments.

The Apparatus for Thermal Transpiration.

This consisted principally of an instrument called a thermo-diffusiometer, of which the essential feature is two chambers, separated by a plate of porous material, means being provided for keeping the chambers at constant, but different, temperatures for many hours at a

time, also for measuring the pressure of gas in the chambers, for exhausting the chambers, and for bringing the chambers into direct communication when required. The different temperatures were secured by a stream of steam on the one side, which gave a temperature of 212° F., and a stream of water on the other side, which gave a temperature constant for the time, but which ranged during the investigation from 47° in February to 70° in July.

The porous plates tried were of biscuit-ware, stucco, and meerschaum, and ranged in thickness from ·06 (1·5 millims.) to ·44 inch (11·2 millims). The pressures of gas within the chambers and the difference of pressure on the two sides of the plate were measured by mercury gauges. A special instrument used for reading the differential gauge read to the $\frac{1}{10000}$th of an inch (·0025 millim.).

Several weeks were spent on this apparatus in getting it tight, getting the gauges to work, and getting rid of the disturbing effects of moisture, before any definite results were obtained, but finally the instrument answered extremely well.

The Experiments on Thermal Transpiration.

The streams of steam and water having been kept going for several hours, long enough for the condition of temperature in the instrument to be perfectly steady, the tap which established communication between the chambers on the opposite sides of the porous plate having been open, so that the pressure in these chambers was equal, this tap was closed, so that the sole communication was through the porous plate. Any difference of pressure between these chambers was then read on the differential gauge.

Supposing that on the first reading the gas (whatever it might be) within the instrument was at the pressure of the atmosphere, a certain quantity of gas was then drawn out and the experiment repeated. This was done until the pressure within the instrument was as low as ·25 inch of mercury.

According to the theoretical deductions, it had appeared that when the sole communication between the two chambers was through the porous plate, and the gas in these chambers was at the same pressure, the difference of temperature would cause the gas to pass from the colder chamber to that which was hotter, until a certain difference of pressure was established, after which there would be no further change as long as the same difference of temperature was maintained, so that the result to be expected as giving evidence of thermal transpiration was a difference in the pressure on the two sides of the plate.

This difference was first obtained with air at the pressure of the atmosphere and a biscuit-ware plate, the difference being ·1 inch (2·54 millims).

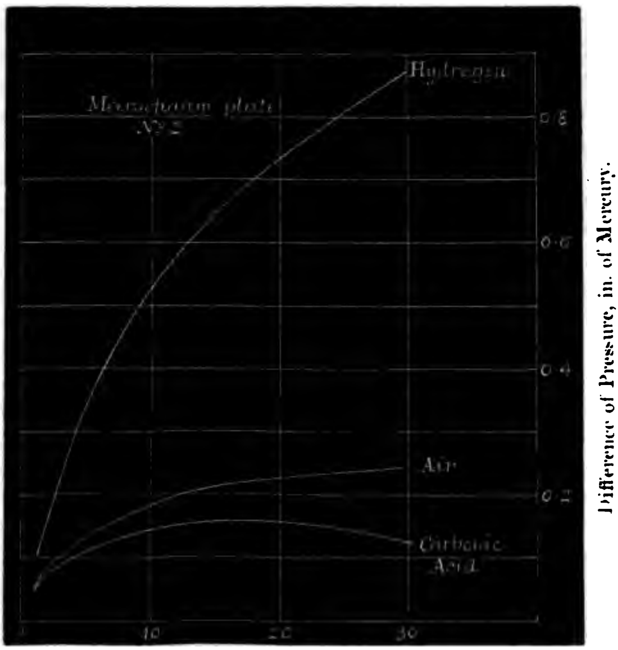
It further appeared from the theory that the difference which would,

cæteris paribus, depend on the difference of temperature, would also depend on the relation between the density of the gas and the coarseness of the plate, so that, *cæteris paribus*, the finer the plate the greater the difference, and this conclusion was at once verified.

A plate of meerschaum, ·25 inch (6·3 millims.) thick, gave a difference, ·25 inch with air, and ·88 with hydrogen, at the pressure of the atmosphere, while a plate of stucco of the same thickness as the meerschaum only gave a difference of ·02 inch with air and ·14 inch with hydrogen.

It also appeared from the theory that with the same plate and the same gas, the difference of pressure should be a maximum at some particular density, so that if the initial density was sufficient, the thermal difference of pressure would increase as exhaustion proceeded up to a certain point and then fall off, the density at which the thermal difference would be a maximum depending on the coarseness of the plate and the nature of the gas. These conclusions were verified.

FIG. 1.



Mean Pressure in inches of Mercury.

For with the meerschaum plate the thermal difference for air was almost constant at pressures nearly equal to the atmosphere, but fell at *an increasing* rate as the density diminished (this is shown by the curve

for air, fig. 1). From this it was clear that if the thermal difference reached a maximum it would be at some pressure greater than that of the atmosphere. With stucco the thermal difference for air increased as the pressure fell from that of the atmosphere, and reached a maximum only at a pressure of about 8 inches of mercury (shown in fig. 2).

FIG. 2.

A comparison of these results shows that the density at which the thermal transpiration is a maximum depends on the coarseness of the plate; and that it depends on the nature of the gas appears at once on comparing the results for hydrogen, air, and carbonic acid, which are shown on figs. 1 and 2.

Experiments with plates of various thicknesses gave the thermal difference of pressures independent of the thickness of the plate, so long as the difference of temperature was the same.

Several minor deductions from the theory were also directly verified.

The Law of Corresponding Results at Corresponding Densities.

In order to establish this law it was necessary to compare the results obtained with different plates. According to the law, the ratio of the thermal difference of pressure to the mean pressure with a particular plate and a particular gas should be the same as with another plate and the same gas, as long as the densities (or pressures) are in a fixed ratio, which is the ratio of the fineness of the plates.

A simple numerical calculation sufficed to show that this conclusion is approximately verified. On dividing the thermal differences by the mean pressure for both the meerschaum and stucco plates, it appears that the resulting numbers are approximately equal, so long as the pressure with the meerschaum is six times as great as with the stucco. This is so both for air and hydrogen, and through a range of pressures from 30 to ·35 inches with the meerschaum, and 5 to ·2 inches with the stucco.

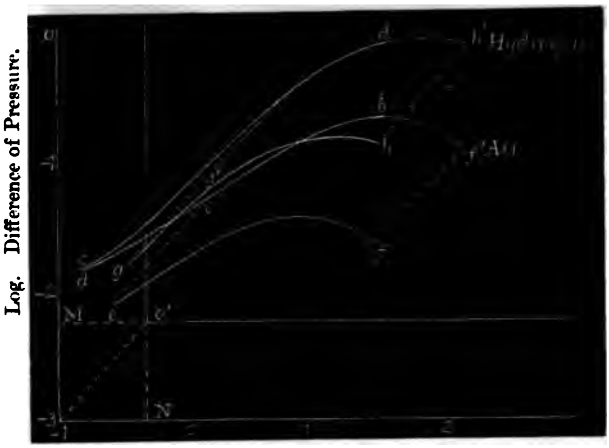
The numerical comparison does not, however, bring out the agreement in nearly as strong a light as the comparison which has been effected by a graphic method.

Graphic Method of comparing the Results.

This method consists in taking as ordinates and abscissæ, not the thermal differences and mean pressures, but the logarithms of these quantities, and the curves so formed are called the logarithmic homologues of the curves shown in figs. 1 and 2.

Any common ratio which exists between ordinates or abscissæ of corresponding points on the natural curves becomes a common difference between the ordinates or abscissæ of their logarithmic homologues, so that if the natural curves correspond after the manner that has just been described, their logarithmic homologues must be precisely similar curves, such that by shifting the one parallel to itself it can be made to fit on to the other.

FIG. 3.



In fig. 3 $a\,b$ and $c\,d$ are the logarithmic homologues respectively for the air curve and the hydrogen curve with meerschaum, and $e\,f$ and $g\,h$ are the logarithmic homologues respectively for the air and hydrogen curves with stucco. By tracing $e\,f$ and $g\,h$ together with their axes on the same paper, and moving the paper without turning it, until the traced curves fit the curves for meerschaum, it is found that the fit is perfect, a portion of the traced curve $e'\,f'$ coinciding with a portion of $a\,b$, and a portion of $g'\,h'$ coinciding with $c\,d$.

O′ M and O′ N, the components of the shift, are the logarithms of the ratios of the corresponding ordinates and abscissæ of the natural curves; and in the particular case to which fig. 2 refers—

$$\text{O}'\text{N} = \cdot 7 = \log. 5$$
$$\text{O}'\text{M} = \cdot 77 = \log. 5\cdot 9$$

It is thus seen that the reason why the numerical comparison did not

give absolutely consistent results is because the ratio of the corresponding abscissæ is not exactly the same as that of the corresponding ordinates, the difference being found on examination to be owing to a discrepancy in the temperatures, which affected the ratio of the corresponding ordinates, but not the ratios of the corresponding abscissæ. These, therefore, give 5 as the ratio of the coarseness of these particular plates.

The woodcut, fig. 3, merely illustrates the result. The logarithmic homologues of the curves for both air and hydrogen, plotted with the greatest care, the points marking the experiments being so close together that it was scarcely necessary to draw a curve, have been compared, and the agreement is very remarkable, the only slight deviation being that shown in fig. 3, which was found to be owing to some impurity in the hydrogen at pressures below an inch of mercury.

In order fully to appreciate the force of this agreement, it must be noticed that it is not only the portions of the curves which overlap that agree in direction, but also the distances between the curves for hydrogen and air, which are shifted in pairs.

Nothing could prove more forcibly than this fitting that the difference in the results for different plates depends on a relation between the density of the gas and the coarseness of the plates.

Experiments on Transpiration under Pressure.

According to the theoretical deductions the rate at which gas would be forced through a tube or porous plate by a difference of pressure bearing a fixed ratio to the mean pressure of the gas in passing, would vary with the mean density of the gas according to a law which would hold with different plates, the corresponding results being obtained at pressures inversely proportional to the diameters of the tubes. The differences in the laws of transpiration which Graham found with different tubes and plates are, so far as they go, in fair accordance with the law as deduced from this theory, but the range of densities over which Graham's results extend is too small to allow a very complete verification, and the chief object in these experiments was to extend this range of densities. The apparatus used was the thermo-diffusiometer, slightly modified, and without the streams of steam and water. The instrument lent itself very well to this part of the investigation. It allowed of the measurement of the time of transpiration of a definite volume of gas, measured at whatever might be the pressure of the instrument, through the porous plate, under a difference of pressure bearing a fixed ratio to the pressure within the instrument.

The times of transpiration of equal volumes of air and hydrogen through plates of stucco and meerschaum were determined at pressures varying from that of the atmosphere to a fraction of an inch of mercury.

These results, in as far as the conditions correspond, were found to agree very closely with the results obtained by Graham. Thus, through stucco at 30 inches, the comparative times of transpiration of air and hydrogen were as 2·9 to 1, Graham's results being 2·8 to 1. Through meerschaum the ratio was 3·6 to 1, Graham having found the ratio 3·8 to 1 through a graphite plate, which was in all probability finer than the meerschaum. The ratio of the times of transpiration of equal volumes of different gases, which Graham looked upon as varying only with the coarseness of the plates, was found, as was expected, to depend entirely on the relation between the pressures of the gases and the coarseness of the plates, the ratio of the times being the same as long as the pressures of the gases were inversely proportional to the coarseness of the plates.

Thus, at a pressure of 5 inches, the times for hydrogen and air through stucco, instead of being 2·9, as at the pressure of the atmosphere, were 3·6, or the same as through meerschaum at a pressure six times as great; the coarseness of the plates, as determined in the previous experiments, being 5·6. The same agreement held as long as the ratio between the pressures was maintained.

The correspondence of the results for different plates, and the complete verification of the theoretical conclusions which they afforded, is shown by comparing the logarithmic homologues of the curves in which the times of transpiration are the ordinates and the pressures the abscissæ. The fitting of the logarithmic homologues is exact, both as regards the direction of the curves and the distances between the curves, for air and hydrogen; the displacement along the abscissæ, to bring the curves into coincidence, being $\cdot 819 = \log. 6\cdot 5$.

As this number, 6·5, is the ratio of the coarseness of the plates, it should have corresponded with the ratio obtained by thermal transpiration, which was 5·6, with the same plates. This discrepancy, although too small to cast a doubt upon the general agreement of the results, is too large to be attributed to experimental inaccuracy, and must have been due to some change in the plates, probably arising from the plates being hot in the one case and cold in the other.

Experiments on Impulsion with a Suspended Fibre.

A single fibre of unspun silk was suspended from one end in a vertical test-tube, closed with an india-rubber cork, and connected with an air-pump, and a microscope was arranged for observing the motion of the fibre when a hot body was brought into a certain position near the test-tube.

With air in the test-tube at the pressure of the atmosphere, it was found that the fibre was carried by the air-currents towards the hot body, and this was the case as long as the pressure was greater than *8 inches of* mercury, but after the tube had been exhausted below

this point, the fibre moved away from the heat, and the motion steadily increased as the pressure became very small, such as $\frac{1}{50}$th of an inch of mercury.

With hydrogen in the test-tube, the fibre moved away from the heat at all pressures below that of the atmosphere, and for small pressures the motion was somewhat larger than with air.

A spider-line was also tried, and gave results similar to the fibre of silk.

It thus appeared that both with the fibre of silk and the spider-line the phenomena of impulsion were manifest at densities many hundred times greater than the highest densities at which like results are obtained with the vanes of the radiometer which are several hundred times broader than the fibre of silk. And this verifies the law of corresponding results at corresponding densities for this class of phenomena.

Abstract of Part II (Theoretical Investigation).

The characteristic as well as the novelty of this investigation is that, not only is the mean of the motions of the molecules at the point under consideration taken into account, but also the manner in which the mean motion may vary from point to point, in any direction across the point under consideration. It appears that such a variation gives rise to certain stresses in the gas (tangential and normal) and it is of these stresses that the phenomena of transpiration and impulsion afford evidence.

Instead of considering only the mean condition of the molecules comprised within an elementary volume of the gas, what is chiefly considered in this investigation is the mean condition of the molecules which cross an elementary area in a plane supposed to be drawn through the point.

Q is used to indicate a quantity belonging to a molecule such as its mass, momentum, or energy, $\sigma_x(Q)$ to express the rate at which Q is carried across a plane perpendicular to the direction x.

Two systems of axes are employed, xyz fixed axes, with respect to which u, v, w, are the component velocities of a molecule, and a system of axes parallel to xyz, but moving with the component velocities U, V, W, with respect to which ξ, η, ζ, have the component velocities of a molecule. U, V, W, having such values that

$$\overline{\xi^2}=\overline{\eta^2}=\overline{\zeta^2}=\frac{a^2}{2}.$$

As preliminary to the investigation, expressions are obtained for $\sigma(Q)$ in terms of a, U, V, and W, on the supposition that the gas is uniform. This is accomplished by the application of well-known methods.

When the condition of the gas varies from point to point, the molecules are considered as consisting of two groups, one crossing from the positive and the other from the negative side of the plane. Considered in opposite directions, the mean characteristics (the number, mass, momentum, or energy) of these two groups are not necessarily equal. They may differ in consequence of the motion of the gas, the motion of the plane through the gas, or a varying condition of the gas, and the determination of the effect of these causes, particularly the last, on the mass, momentum and energy that may be carried across by either or both groups constitutes the extension of the dynamical theory of gases.

In order to take account of the difference in the two groups it is assumed, and so far there is nothing new in the assumption itself, that the group of molecules which crosses the surface from either side will partake of the characteristics of the gas in the region from which the molecules which constitute the group have come. The first direct step in the investigation is the deduction from the foregoing assumption of two theorems (I and II), supposing that there are no external forces. Taking $\sigma'(Q)$ to be the approximate value of $\sigma(Q)$ obtained on the assumption that the gas is uniformly in the mean condition which holds at the point xyz, the theorems I and II admit of the following symbolical expression:—

$$\sigma(Q)=\sigma'(Q)-s\left\{\frac{d}{dx}\sigma'(Q)+\frac{d}{dy}\sigma'(Q)+\frac{d}{dz}\sigma'(Q)\right\}.$$

Where s represents a certain distance, measured from the plane of reference.

This distance, s, enters as a quantity of primary importance into all the results of the investigation.

It is proposed to call s the mean range of the quantity Q, so as to distinguish it from the mean path of a molecule.

s is a function of the mean path, but it also depends on the nature of the impacts between the molecules. It is subsequently shown that—

$$s=\sqrt{\pi}\, a\mu,$$

from which it appears that $s\propto\frac{a}{\rho}$.

The dynamical conditions of steady density, steady momentum, and steady energy are then considered.

Putting $\Sigma(Q)$ for the value of Q in a unit of volume, in order that $\Sigma(Q)$ may be steady, we have—

$$\frac{d}{dx}\sigma_x(Q)+\frac{d}{dy}\sigma_y(Q)+\frac{d}{dz}\sigma_z(Q)=0,$$

whence, by giving to Q the value M, the mass of a molecule, we have

the condition of steady density, for steady momentum Q has severally the values Mu, Mv, Mw, and for steady energy $Q=M(u^2+v^2+w^2)$.

The equations of motion are then applied to the particular cases which it is the object of this investigation to explain. Two cases are considered.

The first case is that of a gas in which the temperature and pressure vary only along a particular direction, so that the isothermal surfaces and the surfaces of equal pressure are parallel planes; this is *the case of transpiration.*

The second case is that in which the isothermal surfaces and surfaces of equal pressure are curved (whether of single or double curvature); this is the case of *impulsion* and the *radiometer.*

Transpiration.

As regards the first case, the condition of steady energy proved to be of no importance, but from the conditions of steady momentum and steady density, an equation is obtained between the velocity of the gas, the rate at which the temperature varies, and the rate at which the pressure varies, the coefficients being functions of the absolute temperature of the gas, the diameters of the apertures, and the ratio of these diameters to the mean range, which coefficients are known for the limiting conditions of the gas, *i.e.*, when the density is either very small or very large.

The most general form of this equation is—

$$\Omega=c\sqrt{\frac{2p}{\rho}}\left\{F_1\left(\frac{c}{s}\right)\frac{1}{p}\frac{dp}{dx}-F_2\left(\frac{c}{s}\right)\frac{M}{\tau}\frac{d}{dx}\left(\frac{\tau}{M}\right)\right\},$$

in which Ω is the mean velocity of transpiration along the tube, which is taken in the direction of the axis of x. M is the mass of a molecule, p the pressure of the gas, τ the absolute temperature, and c the semi-distance across the tube.

$$F_1\left(\frac{c}{s}\right)=\frac{\sqrt{\pi}}{2}\left\{A\frac{c}{s}+m\left[\lambda_1 f_1\left(\frac{c}{s}\right)+\lambda_2 f_2\left(\frac{c}{s}\right)\right]\right\},$$

$$F_2\left(\frac{c}{s}\right)=\frac{1}{4\sqrt{\pi}}\left\{\pi m' f_3\left(\frac{c}{s}\right)+\frac{s}{c}\lambda_3 f_4\left(\frac{c}{s}\right)\right\}\left\{f\left(\frac{c}{s}\right)+\lambda_1 f_1\left(\frac{c}{s}\right)+\lambda_2 f_2\left(\frac{c}{s}\right)\right\}.$$

In which A, m, m' depend only on the shape of the section of the tube.

$f\left(\frac{c}{s}\right)$ is of the order $\frac{c}{s}$ when $\frac{c}{s}$ is zero, and is finite when $\frac{c}{s}$ is infinite,

$f_1\left(\frac{c}{s}\right)$ and $f_3\left(\frac{c}{s}\right)$ are unity when $\frac{c}{s}$ is zero, and zero when $\frac{c}{s}$ is infinite,

and

$f_2\left(\frac{c}{s}\right)$ and $f_4\left(\frac{c}{s}\right)$ are zero when $\frac{c}{s}$ is zero, and unity when $\frac{c}{s}$ is infinite.

All these functions varying continuously between the limits here ascribed. Also—

λ_1 depends on the nature of the surface of the tube, but not upon the nature of the gas, while

λ_2 and λ_3 may depend both upon the gas and the surface.

From this equation, which is the general equation of transpiration, the experimental results, both with regard to thermal transpiration and transpiration under pressure, are deduced.

Impulsion.

In dealing with the second case, that in which the isothermal surfaces are curved, the three conditions—steady density, momentum, and energy—are all of them important.

These conditions reduce to an equation between the motions of the gas the variation in the absolute temperature and the variation in the pressure, with coefficients which involve the ratio of the mean range to the dimensions of the radii of curvature of the surfaces. The equation corresponds to the equation of transpiration, and as applied to the case in which heat is being conducted through a gas which is constrained to remain at rest, the equation becomes—

$$\frac{p-p_1}{p}=\frac{2}{\pi}s^2\frac{\mathrm{M}}{\tau}\frac{d^2}{dx^2}\left(\frac{\tau}{\mathrm{M}}\right),$$

or taking

$$s=\sqrt{\pi}\frac{a\mu}{2p},$$

$$p-p_1=\frac{\mu^2}{\rho}\frac{\mathrm{M}}{\tau}\frac{d^2}{dx^2}\left(\frac{\tau}{\mathrm{M}}\right),$$

$p-p_1$ being the excess of pressure in the direction of, and due to, the variation of temperature. In the abstract of a paper read in April last, Professor Maxwell gives an equation which, transformed into my symbols is—

$$p-p_1=\frac{3\mu^2}{\rho}\frac{\mathrm{M}}{\tau}\frac{d^2}{dx^2}\left(\frac{\tau}{\mathrm{M}}\right),$$

which only differs from mine in the coefficient -3. As Professor Maxwell indicates that he has obtained his result without taking account of the tangential stresses, this difference is not a matter of surprise.

Besides the broad lines of the investigation which have been mentioned in this abstract, there are many minor points of which it is

impossible to convey any adequate idea without going fully into the subject.

Some idea of the scope of the investigation may be gathered from the last section in the paper, which is accordingly introduced here.

Section XIII.—Summary and Conclusion.

Article 125. The several steps of the investigation which have been described may be enumerated as follows:—

(1.) The primary step from which all the rest may be said to follow is the method of obtaining the equations of motion so as to take into account not only the normal stresses which result from the mean motion of the molecules at a point, but also the normal and tangential stresses which result from a variation in the condition of the gas (assumed to be molecular). This method is given in Sections VI, VII, and VIII.

(2.) The method of adapting these equations to the case of transpiration through tubes and porous plates is given in Section IX. The equations of steady motion are reduced to a general equation expressing the relation between the rate of transpiration, the variation of pressure, the variation of temperature, the condition of the gas, and the lateral dimensions of the tube.

In Section X is shown the manner in which were revealed the probable existence (1) of the phenomena of *thermal transposition*, and (2) the law of correspondence between all the results of transpiration with different plates, so long as the density of the gas is inversely proportional to the linear lateral dimensions of the passages through the plates; from which revelations originated the idea of making the experiment on thermal transpiration and transpiration under pressure.

(3.) It is also shown in Section X that the phenomena of transpiration resulting from a variation in the molecular constitution of the gas (investigated by Graham) are also to be deduced from the equation of transpiration.

(4.) The method of adapting the equations of motion to the case of impulsion is given in Section XI.

In Section XII is shown how it first became apparent that the extremely low pressures at which alone the phenomena of the radiometer had been obtained were consequent on the comparatively large size of the vanes, and that by diminishing the size of the vanes similar results might be obtained at higher pressures, whence followed the idea of using the fibre of silk and the spider-line in place of the plate vanes.

(5.) In Section XII it is also shown that while the phenomena of the radiometer result from the communication of heat from a surface to a gas, as explained in my former paper, these phenomena also depend on the divergence of the lines of flow, whence it is shown that all the peculiar facts that have been observed may be explained.

(6.) Section II, Part I, contains a description of the experiments undertaken to verify the revelations of Section X respecting *thermal transpiration*, which experiments establish not only the existence of the phenomena, but also an exact correspondence between the results for the different plates at corresponding densities of the gas.

(7.) Section III contains a description of the experiments on *transpiration under pressure* undertaken to verify the revelations of Section X with respect to the correspondence between the results to be obtained with plates of different coarseness at certain corresponding densities of gas, which experiments proved not only the existence of this correspondence, but also that the ratio of the corresponding densities in these experiments is the same as the ratio of the corresponding densities with the same plates in the case of thermal transpiration—a fact which proves that the ratio depends entirely on the plates.

(8.) Section IV contains a description of the experiments with the fibre of silk, and with the spider-line undertaken to verify the revelations of Section XII, from which experiments it appears that, with these small surfaces, phenomena of impulsion, similar to those of the radiometer, occur at pressure but little less than that of the atmosphere.

Conclusion.

Article 126. As regards transpiration and impulsion, the investigation appears to be complete; most, if not all, the phenomena previously known have been shown to be such as must result from the tangential and normal stresses consequent on a varying condition of a molecularly constituted gas; while the previously unsuspected phenomena to which it was found that a variation in the condition of gas must give rise, have been found to exist.

The results of the investigation lead to certain general conclusions which lie outside the immediate object for which it was undertaken; the most important of these, namely, that gas is not a continuous plenum, has already been noticed in Article 5, Part I.

The Dimensional Properties of Gas.

Article 127. The experimental results considered by themselves bring to light the dependence of a class of phenomena on the relations between the density of the gas and the dimensions of the objects owing to the presence of which the phenomena occur. As long as the density of the gas is inversely proportional to the coarseness of the plates the transpiration results correspond; and in the same way, although not so fully investigated, corresponding phenomena of impulsion are obtained as long as the density of the gas in inversely *proportional* to the linear size of the objects exposed to its action;

in fact, the same correspondence is found with all the phenomena investigated.

We may examine this result in various ways, but in whichever way we look at it, it can have but one meaning. If in a gas we had to do with a continuous plenum, such that any portion must possess the same properties as the whole, we should only find the same properties, however small might be the quantity of gas operated upon. Hence, in the fact that we find properties of a gas depending on the size of the space in which it is enclosed, and on the quantity of gas enclosed in this space, we have proof that gas is not continuous, or, in other words, that gas possesses a dimensional structure.

In virtue of their depending on this dimensional structure, and having afforded a proof thereof, I propose to call the general properties of a gas on which the phenomena of transpiration and impulsion depend, the *Dimensional Properties of Gas.*

This name is also indicative of the nature of these properties as deduced from the molecular theory; for by this it appears that these properties depend on the mean range, a linear quantity which, *cæteris paribus*, depends on the distance between the molecules.

In forming a conception of a molecular constitution of gas, there is no difficulty in realizing that there must exist such dimensional properties; there is, perhaps, greater difficulty in conceiving molecules so minute and so numerous that in the resulting phenomena all evidence of the individual action is lost; but the real difficulty is to conceive such a range of observational power as shall embrace, on the one hand, a sufficient number of molecules for their individualities to be entirely lost, while, on the other hand, it can be so far localized as regards time and space, that, if not the action of individuals, the action of certain groups of individuals, becomes distinguishable from the action of the entire mass. Yet this is what we have in the phenomena of transpiration and impulsion.

Although the results of the dimensional properties of gas are so minute that it has required our utmost powers to detect them, it does not follow that the actions which they reveal are of philosophical importance only; the actions only become considerable within extremely small spaces, but then the work of construction in the animal and vegetable worlds, and the work of destruction in the mineral world, are carried on within such spaces. The varying action of the sun must be to cause alternate inspiration and expiration, promoting continual change of air within the interstices of the soil as well as within the tissue of plants. What may be the effect of such changes we do not know, but the changes go on; and we may fairly assume that, in the processes of nature, the dimensional properties of gases play no unimportant part.

II. "Absorption of Gases by Charcoal. Part II. On a new Series of Equivalents or Molecules." By R. ANGUS SMITH, Ph.D., F.R.S. Received January 30, 1879.

(Abstract.)

In the "Transactions of the British Association," 1868, Norwich, on page 64 of the "Abstracts," there is a preliminary notice of an investigation into the amount of certain gases absorbed by charcoal. I made the inquiry from a belief previously expressed in a paper of which an abstract is in the "Proceedings of the Royal Society," page 425, for 1863. I said in that paper that the action of the gas and charcoal was on the border line between physics and chemistry, and that chemical phenomena were an extension of the physical; also that the gases were absorbed by charcoal in whole volumes, the exceptions in the numbers being supposed to be mistakes. The results given were:—

Hydrogen	1
Oxygen	7·99
Carbonic oxide	6·03
Carbonic acid	22·05
Marsh-gas	10·01
Nitrous oxide	12·90
Sulphurous acid	36·95
Nitrogen	4·27

It was remarked that the number for nitrogen was probably too low; I had some belief that the charcoal retained a certain amount which I had not been able to estimate.

For common air, the number 40·065 crept into the paper or abstract instead of the quotient 7·06.

I considered the numbers very remarkable, but was afraid that they would be of little interest unless they could be brought more easily under the eyes of others; my experiments were somewhat laborious; the exact numbers were seldom approached by the single analysis, but were wholly the result of a series of irregular averages and apparently irregular experiments. The cause of this was clear, as I believed, namely, the irregular character of the charcoal with which I had to deal. The experiments which I had published were forgotten, I suppose, by most men, but the late Professor Graham told me that he had repeated them with the same results which I had given. I might have considered this sufficient, but waited for time to make a still more elaborate investigation of the subject, and to take special care with oxygen, in the belief that, the rule being found,

the rest of the inquiry would be easy; this was extended to nitrogen, but not by so many experiments as with oxygen. I am now assured of a sound foundation for inquiries, which must take their beginning from the results here given.

It is found that charcoal absorbs gases in definite volumes, the physical action resembling the chemical.

Calling the volume of hydrogen absorbed 1, the volume of oxygen absorbed is 8. That is, whilst hydrogen unites with eight times its weight of oxygen to constitute water, charcoal absorbs eight times more oxygen by volume than it absorbs hydrogen. No relation by volume has been hitherto found the same as the relation by weight.

The specific gravity of oxygen being 16 times greater than hydrogen, charcoal absorbs 8 times 16, or 128 times more oxygen by weight than it does hydrogen. This is equal to the specific gravity of oxygen squared and divided by two $\frac{16^2}{2}$, or it is the atomic weight and specific gravity multiplied into each other, 16×16, and divided by two $\frac{256}{2}=128$.

Nitrogen was expected to act in a similar way, but it refused. The average number of the latest inquiry is 4·52, but the difficulty of removing all the nitrogen from charcoal is great, and I suppose the correct number to be 4·66. Taking this one as the weight absorbed, $14 \times 4{\cdot}66 = 65{\cdot}3$, or it is $\frac{14^2}{3}$. Oxygen is a dyad; nitrogen a triad.

We have then carbonic acid not divided, but simply 22 squared $=484$.

Time is required for full speculation, but the chemist must be surprised at the following:—

Carbonic oxide	6	volumes.	
Carbonic acid, CO_2	6+16	„	=22
Marsh-gas, CH_4	6+4	„	=10
Protoxide of nitrogen, NO......	8+4·66 (N)	(4·9)	12·466.

These four results belong to the early group not corroborated lately, but so remarkably carrying out the principle of volume in this union giving numbers the same as those of weight in chemical union, that they scarcely require to be delayed.

I am not willing to theorize much on the results; it is here sufficient to make a good beginning. We appear to have the formation of a new series of molecules made by squaring our present chemical atoms, and by certain other divisions peculiar to the gases themselves. Or it may be that the larger molecule exists in the free gas, and chemical combination breaks it up. These new and larger molecules may lead us to the understanding of chemical combinations in organic chemistry,

and whenever there is union not very firm, and may also modify some of our opinions on atomic weights and the motion of gases.

Of course, I cannot pretend to give the result of these results; but as we have here the building up of a molecule by volumes, so as to form an equivalent of physical combination analogous to the chemical equivalent, it is impossible to avoid seeing that it indicates the possibility of our present equivalents being made up in a similar manner.

I did not expect these numbers; but I certainly, as my previous paper showed, had in full view a necessity for some connexion between physical and chemical phenomena more decided than we possessed.

February 13, 1879.

W. SPOTTISWOODE, M.A., D.C.L., President, in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "Note on the Development of the Olfactory Nerve and Olfactory Organ of Vertebrates." By A. MILNES MARSHALL, M.A., D.Sc., Fellow of St. John's College, Cambridge. Communicated by W. S. SAVORY, F.R.S., Surgeon to and Lecturer on Surgery at St. Bartholomew's Hospital. Received January 30, 1879.

In the course of an investigation into the development of the cranial nerves of the chick, certain facts came to light indicating that the olfactory nerve, instead of being, as usually described, a structure differing totally in its mode of origin from all the other nerves in the body, in reality "exactly corresponds in mode of development and in appearance with the other cranial nerves, and with the posterior roots of the spinal nerves."*

The present paper contains the results of further investigations on this point; it deals also with some features in the development of the vertebrate olfactory organ, and with certain questions of a more general nature affected by the conclusions arrived at.

* "*Proc. Roy. Soc.*," vol. xxvi, p. 50, and "Quarterly Journal of Microscopical *Science*," *January*, 1878, pp. 17–23.

The Development of the Olfactory Nerve.

For the sake of clearness the more important conclusions are stated in the form of propositions:—

a. The olfactory nerves do not arise from the cerebral hemispheres, but from the single unpaired forebrain.

In chick embryos of about the fiftieth hour, or a little older, the olfactory nerves can be clearly recognized arising from the upper part of the sides of the forebrain. At this stage there is no trace whatever of the cerebral hemispheres. The olfactory nerves then come into existence before the cerebral hemispheres, and therefore cannot be derived from them. The hemispheres are developed in the chick as lateral outgrowths from the upper part of the forebrain; at first the olfactory nerves have no connexion with them, beyond that of close proximity; but very soon the hemispheres, by their rapid growth forwards, drive the nerves down to the base of the brain, and so make the nerves appear to arise from their under and anterior part.

This account is confirmed in all points by observations on duck embryos, which show clearly that the connexion of the olfactory nerves with the cerebral hemispheres is not of a primary but of a secondary or adaptative nature.

In the dogfish (*Scyllium*) the forebrain is, as has been already shown by Balfour,* single and unpaired up to stage O, presenting till then no trace whatever of a division into cerebral hemispheres: the olfactory nerves are, however, well developed structures by stage M; at which period they can be seen, in transverse sections through the anterior part of the head, arising from the upper part of the sides of the forebrain and running downwards to the olfactory pits. The nerves can be recognized, though with less distinctness, at still earlier stages.

The olfactory nerves of the salmon and of the trout can, in a similar manner, be identified before the cerebral hemispheres have come into existence; and the same statement applies to the axolotl.

b. There is no trace of an olfactory lobe in the early stages of development of the olfactory nerve.

Since the olfactory lobes are commonly described as "hollow outgrowths of the cerebral hemispheres," and the olfactory nerves have just been shown to arise quite independently of the cerebral hemispheres, this second proposition is in reality already proved by the first. However, as the existence of olfactory lobes has been supposed to separate the olfactory from the other cranial nerves, it becomes necessary to investigate carefully the time and conditions of their appearance.

In the chick the olfactory nerve is in its early stages solid, and

* "Elasmobranch Fishes," p. 178.

from a histological point of view differs in no appreciable respect from the other cranial nerves at corresponding stages of their development. At the end of the sixth day of incubation the nerve, which is now of some length, has acquired its secondary connexion with the cerebral hemisphere in the manner described above; yet the nerve is still solid along its whole length, and presents no trace whatever of an olfactory lobe, or hollow outgrowth from the brain. By the end of the seventh day a very small conical pit is visible in the wall of the cerebral hemisphere at the point of origin of the olfactory nerve. This pit, which is the earliest rudiment of the olfactory lobe, is formed almost entirely at the expense of the inner wall of the hemisphere, so that there is hardly any corresponding projection on the outside of the brain.

The development of the olfactory lobe in the dogfish closely resembles that in the chick: at stage M there is no trace whatever of a lobe, though the olfactory nerves are large and conspicuous structures. At a stage a little younger than Balfour's stage O, the first rudiment of an olfactory lobe appears, as a slight lateral bulging of the side of the forebrain, at the point of origin of the olfactory nerve: this increases rapidly, much more so indeed than the nerve itself; by stage Q it is a tolerably prominent structure, and in the later stages it becomes considerably larger than the nerve proper.*

Stage O in the development of a dogfish embryo corresponds to about the sixth day in the chick, so that there is a close agreement in time as well as in mode of development of the olfactory lobe in these two types. In the dogfish, however, the olfactory lobes appear before the cerebral hemispheres are differentiated, and consequently arise from the forebrain; while in the chick the hemispheres are developed rather earlier, and the olfactory lobes arise as direct outgrowths from them, and not from the original forebrain.

In the salmon and trout, from the earliest period at which the existence of an olfactory nerve can be recognized up to the time of hatching, and indeed for some little time afterwards, there is no trace of an olfactory lobe.

The existence of an olfactory nerve without any trace of an olfactory lobe has also been established in the earlier embryonic stages of the axolotl, of the common frog, and of the green lizard.

The olfactory nerve of an adult vertebrate is commonly described as consisting of three parts, a proximal element or *olfactory tract*, an intermediate *olfactory bulb*, and a distal *olfactory nerve proper*, the two former of these corresponding to the olfactory lobe or vesicle of the embryo. From the descriptions given above it would appear that the

* Cf. Balfour, *op. cit.*, p. 178, and Plate 15, figs. 2 and 8*a*. Balfour has not observed the olfactory nerves earlier than stage O, and therefore describes them as *outgrowths from* the olfactory lobes.

third of these elements—the olfactory nerve proper—is the earliest to be developed, and that the olfactory tract and bulb, when present at all, do not appear till an exceedingly late period of development—a period so late indeed that their ultimate presence affords no ground whatever for separating the olfactory from the other cranial nerves.

c. The olfactory nerve is a primary nerve, comparable to the segmental cranial nerves.

Certain of the cranial nerves, *e.g.*, the facial and glossopharyngeal, have long been recognized as possessing segmental value. These segmental nerves in the early stages of their development possess certain characters in common, which serve to distinguish them sharply from other nerves or branches of nerves; of these characters the following are the most important:—(1) They appear very early; (2) they arise (at least in the chick) from the neural ridge on the mid-dorsal surface of the brain; (3) shortly after their appearance their roots undergo a shifting downward of their points of attachment, so that they no longer arise from the dorsal surface, but from the sides of the brain; (4) they present, at least in their early stages, ganglionic enlargements on, or close to, their roots of origin; (5) their course is at right angles to the longitudinal axis of the head; (6) and, finally, they have very definite relations to the segments as indicated by the visceral clefts, each nerve supplying the two sides of a cleft.

In all these points the olfactory nerve agrees very closely with the segmental nerves:—(1) It appears very early in all the types examined, and in the chick it seems to be one of the very first nerves in the body to be developed; (2) there is also strong reason for thinking that, in the chick, the olfactory nerve is developed from the neural ridge;* (3) its point of attachment to the brain undergoes a shifting of precisely similar nature to that presented by the segmental nerves; (4) its direction is at right angles to the longitudinal axis of the head, so that were the cranial flexure to be corrected, and the head straightened out, the course of the olfactory nerve would be parallel to that of the segmental nerves; (5) it possesses a ganglionic enlargement at its point of origin from the brain; (6) and, finally, an attempt will be made in the second part of this paper to show that it supplies the two sides of a visceral cleft.

Since, then, the olfactory nerves do not differ embryologically in any material respect from the segmental cranial nerves, they must be regarded as the first or most anterior pair of true segmental cranial nerves.

The Development of the Olfactory Organ.

This will, in the absence of figures, be treated very briefly; those

* For a discussion of this point, *vide* "Quart. Journ. Micro. Science," January, 1878, pp. 18, 19.

points only being noticed which are of special interest in connexion with the conclusions arrived at in the preceding part of the paper.

The olfactory pits appear at almost the same time as the visceral clefts; or, to speak more accurately, they first become conspicuous objects at, or very shortly after, the time when the anterior visceral clefts become open to the exterior. This occurs about stage K in the dogfish, and about the fiftieth hour in the chick.

In their early stages the olfactory pits present a striking resemblance to the visceral clefts in position, shape, size, and general relations; their external apertures elongate and become slit-like, and the direction of the slit, like that of the visceral clefts, is at right angles to the longitudinal axis of the head. These facts are best illustrated by the study of whole embryos, and of longitudinal vertical sections.* They come out with great clearness in all the types of vertebrates examined, but with especial distinctness in the axolotl and salmon.

The development of the Schneiderian folds presents several points of great interest, which can be most favourably studied in the Elasmobranchs. Attention has already been directed by Balfour† to the very early appearance of these folds. The important point, so far as the present question is concerned, is that these Schneiderian folds appear at the same time as, or very shortly after, the first rudiments of the gills. In addition to this identity in time, there is also identity in structure; in both cases development consists in the formation of a series of equal, closely apposed folds, mainly epithelial, but involving the underlying mesoblast to a certain extent. These folds are in the two cases—gills and Schneiderian folds—of the same width, the same distance apart, have epithelium of the same thickness and same histological character, involve the mesoblast to exactly the same extent, and in exactly the same manner; in a word, are structurally identical.

In the later stages the Schneiderian folds, like the gills, receive a very abundant supply of blood-vessels; and the relations of these vessels to the folds, which are very peculiar and characteristic, are identical in the two cases. Even in the adult Elasmobranch there is a remarkable histological resemblance between the gills and the nose.

The facts above recorded concerning the development of the olfactory nerve and olfactory organ point towards the same conclusions as to morphology of these structures, viz., that the olfactory organ is the visceral cleft; that the olfactory nerve is the segmental nerve supplying that cleft in a manner precisely similar to that in which the hinder

* For figures of whole embryos illustrating the points referred to, *vide* Parker, "On the Structure and Development of the Skull in Sharks and Skates," "Trans. Zool. Soc.," vol. x, part iv, 1878, Pl. 25, fig. 1; Pl. 39, figs. 1 and 2; Pl. 40, fig. 1; and Balfour, *op. cit.*, Pl. 7, Stage L.

† *Op. cit.*, p. 184, and Pl. 44. fig. 14.

clefts are supplied by their respective nerves; and that the Schneiderian folds are gills.*

These conclusions, if accepted, will considerably simplify our conception of the segmentation of the vertebrate head. As there are no nerves or clefts in front of the olfactory segment, the olfactory nerve must be taken as the most anterior nerve, and the nose as the most anterior cleft. The next cleft is that in front of the maxillo-palatine arch, of which a part probably persists in the adult as the lachrymal duct: the segmental nerve corresponding to this cleft is the *third*, or oculomotor nerve. Next comes the mouth cleft, supplied by the *fifth*, or trigeminal, nerve; and then in succession the clefts supplied by the facial, glossopharyngeal, and pneumogastric nerves. This view of the constitution of the vertebrate head is found to accord well with the later researches of Professor Parker on the morphology of the skeletal elements of the head.

Some at least of the labial cartilages will probably prove, on this view, to be homologues of the extrabranchials, a comparison that has already been made by Professor Parker.†

If the olfactory organs are visceral clefts, they must originally have communicated with the mouth cavity. Indications of a former connexion of this kind are by no means wanting; thus in salmon embryos the alimentary canal extends forwards, so as to underlie the nasal sacs: as development proceeds, this anterior prolongation of the mouth cavity gradually shrinks; it persists for a short time as a pair of cæcal diverticula, which ultimately disappear altogether.

In conclusion, it may be noted that the Schneiderian folds afford an instance, on the theory here maintained, of structures originally hypoblastic in nature becoming, from changed circumstances, epiblastic.

II. "On the Development of the Skull and its Nerves in the Green Turtle (*Chelone midas*), with Remarks on the Segmentation seen in the Skull of various types." By Professor W. K. PARKER, F.R.S. Received February 3, 1879.

In the first paper on the development of the skull of the Vertebrata, published in "Phil. Trans.," I figured and described certain modifications of the skull in the embryos of the African ostrich, which have only received their explanation lately, and this has become possible through what I see in the embryos of the green turtle.

For these embryos I am indebted to two of our Fellows, namely,

* Cf. Dohrn, "Ursprung der Wirbelthiere," p. 23.

† "Proc. Zool. Soc.," vol. x, part iv, 1878, p. 212.

Sir Wyville Thomson and Mr. H. N. Moseley; the latter (who made the collection in the Island of Ascension), sending me the smaller specimens, and the former the ripe, and nearly ripe young.

Through the liberality and kindness of these gentlemen, I am put in possession of an invaluable series of specimens, several dozens in number; the smallest being only half an inch long measured along its curve.

Sir Wyville Thomson, having accepted my offer of the memoir I am preparing on this type of reptile for the "Challenger" Series of papers, I am anxious to lay before the Royal Society some at least of the results which I have obtained; so that there may be a connexion kept up between my papers, and this slow ingathering of results be garnered in known places, for the benefit of those who will sift and use them.

I have for many years been familiar with the existence of both paired and unpaired elements in the spine and hinder part of the skull: and also with the *three* cartilages that build up the fore part of the chondro-cranium.

My attention, however, having been directed most to the symmetrical *pro-chordal* bands,—the middle and fore part of the trabeculæ cranii,—the anterior azygous cartilage, although always before my eyes, has never, until lately, received the attention it deserves.

In my first paper (on the skull of the ostrich tribe) I held views with regard to the trabeculæ which I hold now; but there has been an intermediate period in which I have fallen into, what I must now consider to be a serious error.

This error lay in the placing, both by Professor Huxley and myself, of the trabeculæ cranii in the category of visceral arches.

Both of us have known for many years that the hinder part of the trabeculæ of the newt are *para-chordal*, and I more recently discovered that the hinder part of the basi-cranial plate is developed separately in the Amphibia.

Professor Huxley restricts the term "para-chordal" to these hinder plates; I am satisfied that the term must have a wider application.

Nevertheless, what the Selachians, and all types above the Ichthyopsida show, satisfies me that Rathke was right in considering the trabeculæ to be mere continuations of the moieties of the basal plate or "investing mass."

I quite agree with Mr. Balfour in looking upon the whole of these tracts, right and left, to be the undivided representatives of the paired neural arches of the spinal region, where, as a rule, they are distinct from each other, and are developed between the spinal nerves, being inter-segmental.

I am satisfied that dying out of the notochord in front does not affect the real nature of the *pro*-chordal tracts.

Until I understood the development of the pituitary body, its rela-

tion to the notochord was considered by me to be that of direct obstruction to the forward growth of that rod.

I now, see, however, that this azygous structure, the true skeletal axis, which is early separated from the hypoblast, begins to starve at its fore end, before the pituitary body is formed.

The main part of the hypoblast, as a rule, is arrested just behind the oral involution; and above that point the notochord ceases to grow.

At first, as Mr. Balfour shows, this rod is hooked downwards in front, for it follows exactly the curve of the mid-brain.

There is, however, at the time of its arrest, no physical obstacle to stop its growth still further forwards beneath the fore-brain, to the utmost limit of the frontal wall of the embryo.

Thus the primary skeletal axis stops and shrinks by what may be called an anticipation of the obstructing wall that will be.

Here is something in morphology resembling hereditary instinct in zoology.

The segmentation of the embryo is at first altogether that which is seen in the somatomes, and after these have become converted into the muscle-plates a new alternating segmentation takes place, so that the short muscular bands can work and produce the vermicular contractions of the embryo.

The tissue which becomes the vertebræ is primarily marked out into serial parts in conformity with the segmental cell masses that form the muscle-plates.

Very soon, however, a new segmentation takes place, and the primordial vertebræ are intermediate to the muscular masses.

This secondary, intercalary vertebrate segmentation is very slightly developed in the head.

For a good while in all the Vertebrates, and permanently in some, the mesoblastic sheath of the notochord becomes a continuous cartilage; in all but the lower forms this undergoes segmentation to form the "bodies" of the vertebræ.

In the head, as a rule, this second sheath of the notochord is but little developed, and has a very slight degree of separateness from the investing paired cartilages—para-chordals, and hinder part of the trabeculæ.

In the Selachians, however, it is well developed, and in them the distinction between head and body by means of the occipito-atlantal articulation is *late;* in some Batrachians, notably in the huge tadpoles of *Pseudis paradoxa*, before the limbs are grown, this cartilaginous sheath of the notochord is large and thick.

The distinction between the hind and fore parts of the skull is greater by far than the distinction between the vertebral column and the hind part of the head.

For this latter region has, in common with the spine, the notochord, its mesoblastic sheath, paired neural and hæmal elements, besides a hypoblastic lining to the digestive tube beneath.

Moreover, there is a great tendency to produce vertebræ in the hind part of the head in some Vertebrata; in several of the Urodelous Amphibia there are three rudimentary vertebræ in front of the "atlas."

Thus the scant growth of the hypoblast in the cephalic region of the blastoderm, would appear to be one of the causes of the extreme modification of the head as compared with the spinal column.*

We shall have made a great stroke in embryology when we have explained the peculiar behaviour of the epiblastic covering of the fore-part of the head.

There, the oral involution, which is formed from epiblast, turns inwards and upwards into the fore-part of the hollow under the mid-brain, and grafts itself upon the back of the down-turned fore-brain, itself of epiblastic origin; thus the pituitary body is formed.

In all available interspaces the mesoblast of the fore half of the head grows in and forms the supporting structures, and the vascular supply.

But now there arises this question—are the mesoblastic structures of the cranium in the *pro*-chordal region perfectly homologous with those in the *para*-chordal, whether cephalic or spinal?

In endeavouring to answer this question, I must return to the point at which I started, where it was mentioned that there existed an unpaired pro-chordal cartilage between the symmetrical trabeculæ.

This, in the fore-part of the chondro-cranium, is a familiar part—pre-nasal rostrum ("Ostrich's Skull," Plate 7, *p*, *n*); its largest development is in the skate, saw-fish, and whale.

But the perpendicular ethmoid and septum nasi are, in reality, other parts of the same azygous cartilage (see "Frog's Skull," Plate 6, figs. 9, 10); in the Batrachia I have studied the development of this median part in a large number, both of individuals and species.

But its real character is best seen in my *second* and *third* stages of the embryos of *Chelone midas;* these measured along their curves are respectively two-thirds of an inch and one inch and a quarter in length.

In the younger of these the trabeculæ are like those of the frog and Selachian, but they stop short and end in a somewhat out-turned "cornu" behind the nasal sacs; they are flat in front and rounded behind.

* Since the above was written, Dr. Milnes Marshall has shown me that in the embryo of the salmon the hypoblast, *at first*, runs forwards to the nasal sacs, and ends in a blind cavity behind the frontal wall in the fore part of the palate. One of my own figures shows this ("Salmon's Skull," Plate 2, fig. 10); I noticed and *figured this upper* pre-oral recess, but could not interpret its meaning.

Between these there is another (azygous) rod; this passes between the trabeculæ behind, it lies on a lower plane, and ends where the front of the pituitary body *will be*, for this part is only forming as yet.

In front this rounded rod runs forwards to the anterior wall of the head; not so the trabeculæ, for they, unlike what is seen in the Batrachia, are behind the nasal sacs, and never pass far into that region.

In the frog, and his congeners, they form a broad floor to the nasal labyrinth.

At any earlier stage than this the notochord with at least a film of mesoblast, *ensheathing the intrinsic* sheath, might have stretched itself to the point where this pre-pituitary rod ends behind; but it took an upward course, and, following the curve of the mid-brain, turned down again, and then stopped short.

I cannot see what other explanation of this *solid rod* can be given than that it is the true homologue of the mesoblastic cartilaginous sheath of the notochord; it is solid because it has nothing to enclose.

Mr. Balfour is quite right in saying that the mesocephalic flexure is only apparently recovered from; the head, indeed, has its axis permanently shortened by this bend of the mid-brain on itself.

Indeed, the skull is shortened by this, as a dress is shortened by having a "tuck" taken up in it; or as a river shortens itself when it cuts out a new channel at the base of a sharp bend.

The "post-pituitary wall" lies in the axis of the arched space formed by the bend of the mid-brain on itself; it is extremely large in the embryo of the green turtle.

It must be considered that the *frontal wall* is not the *organic* end of the embryo, but the upper surface of that end.

The fore-brain looks directly downwards, and even a little backwards; the olfactory nerves arise from its anterior (= superior) surface, and the "infundibulum" buds out to meet the oral involution on the posterior (= inferior) surface of the fore-brain.

We thus see that the true organic *punctum terminale* must lie between these two parts, the olfactory nerve and infundibulum, and therefore it looks downwards, and a little backwards.

So when the trabeculæ cranii grow to the frontal wall they in reality turn upwards, and embrace the dorsal region of the front of the head.

Thus, it is evident that if we can trace the notochord to the back of the pituitary body, we have found it reaching, very nearly, the fore-end of the embryo.

Now, in my third stage of *Chelone midas* the notochord turns round in the post-clinoid upgrowth of the basal plate, and the sheath in its descending part becomes solid, and ends behind the lobules of the rudimentary pituitary body as a tear-shaped drop or lump of cartilage.

If the head had been straight, this drop of cartilage would have reached its fore-end directly below the *first nerves.*

I shall return to this part of the subject in the latter half of my paper.

The visceral or inferior arches of the head are as diverse from the costal arches as the axial parts of the head are from those of the body.

That splitting of the mesoblast, which forms the "body-cavity," runs high up into the muscle-plates; but this upper part of the cavity closes again, whilst, below, the ventral wall is permanently divided into "splanchnopleure" and "somatopleure."

I have corroborated Mr. Balfour's account of the extension of this cavity into the head of the embryo of the Selachians by demonstrating it in the head of the embryo lizard and turtle.

To me this "head cavity" appears to be the equivalent of the temporary upper part of the body cavity; this cephalic extension of the cavity is also temporary.

The cells lining these cavities in the head become transformed into muscular fibres.

Thus, there is a reversion of the ventral wall of the head into a generalised condition before the visceral rods are developed.

In the trunk the axial skeleton* is formed inside the upper diverticulum of the body cavity, and therefore in the *splanchnic* layer; of course the costal arches pass over the permanent body cavity into the *somatic* layer.

Mr. Balfour's view with regard to the visceral arches and branchial pouches is, that they are all formed in a tract equivalent to the somatic layer of the body.

Yet the difference between the costal and visceral arches is very great; and the fact that there are two sets of them, external and internal, separated by a large *branchial space*, does not lessen the difficulty of harmonising these two sets of arches, the costal and the visceral.

We may, however, keep the term *pleural* for both, and divide this "genus" into two "species"—visceral and costal—and the visceral into two varieties, namely, external and internal.

I suspect that there has been a *secular* differentiation of these regions, and that the order in time has been—first, "extra-branchials" round the huge branchial pharynx; then the trabeculæ as a support to the swelling neural axis; then the paired neural cartilages of the spine; after these, the intra-branchials; then the costal arches; and, last of all, the limb-girdles and limbs.

These deductions are not made at random, but by reflection upon

* See Balfour, "Elasmobranchs," p. 133.

the order of time in the appearance of these parts in the *quasi-ancient* stages of the early embryos of existing, but low, vertebrate types.

The ventral wall of the head undergoes dehiscence in *three* places, on each side, in front of the tympano-eustachian, or so-called first cleft.

The right and left clefts directly in front of that open freely into each other below; they form the angles of the opening mouth.

Another cleft, the lacrymal, is formed in the region over which the third nerve forks; this nerve, the *motor oculi*, is a true segmental nerve, but is specially devoted to the eyeball.

The eyeball forms for itself a nest above and in front of the mouth, and this cupped orbital space is permanently open antero-inferiorly.

In the turtle, at my second stage, the maxillo-palatine fold is very large, is dilated at both ends, and pinched in the middle; in the hind part there is for a time, as I have showed in the lizard, an extension of the pleuro-peritoneal cavity.

This is the only one of the *head* cavities which opens into its fellow of the opposite side; the presence of this cavity is as sure a sign of segmentation as the forking of a segmental nerve over the space in front of and above it.

The thick front part of the visceral fold between the lacrymal and nasal clefts does not acquire a cavity.

The whole of this double fold over and in front of the mouth cleft is largely aborted in the Selachians by a foregrowth of the mandibular fold; but in some sharks, as in *Notidanus*, and in all rays, a true palatine cartilage is developed in that part of the face which is between the lacrymal and nasal clefts.*

Between the lacrymal and oral clefts, and therefore between the hinder fork of the third nerve and the *true anterior* fork of the fifth nerve, a visceral cartilage appears in several types.

In *Scymnus*, among the sharks, in *Menopoma* and *Siredon*, among the Urodeles, in nearly all lizards, and in the Chelonians, a separate cartilage appears in the hinder lobe of the maxillo-palatine fold, after the disappearance of the head cavity.

In the lamprey, and the larvæ of the Batrachia, the extreme forward

* This cartilage is distinct also in all the Urodeles; and in the species of the genus *Bufo*, after metamorphosis, and in all the rest of the Batrachians as a very definite region of the *sub-ocular arch*. In the salmon, among the Teleostei, this cartilage is separately developed at first, and this is evidently the rule in the Order.

In the Siluroids (*Doras*, *Clarias*) this becomes a straight rod of bone, carrying the tentacle-bearing, minute maxillary, in front, and lying over the pterygoid, and mesopterygoid behind; with these it never unites.

In lizards, and many birds, there is a very definite palatine cartilage which appears between the lacrymal and nasal clefts; it remains cartilaginous in the former, but in many birds (*Musophaga*, *Dicholophus*, *Diomedea*, &c.) acquires its own bony centre.

extension of the dorsal part of the mandibular arch suppresses the pre-oral cartilages; in the Selachians—sharks especially—this is done by the pterygoid outgrowth of the same element.

In the first case, the whole mouth is dominated by the suctorial cartilages; in the second, the immense development of the pterygoid process of the manibular pier, antagonising the movable lower jaw, causes the great suppression of the *proper* maxillo-palatine fold.

Whilst I consider the trabeculæ to be axial, or rather *neuro-axial*, I am very familiar with the rudiments of a visceral arch growing from their fore ends.

I first saw this in metamorphosing larvæ, and adult, bull-frogs; afterwards Professor Huxley found and described them in *Rana temporia* ("Encyc. Brit., vol. ix, Art. *Amphibia*, p. 755). Since then, I have found them in many kinds of Batrachia; in some Urodeles; in the embryos of the dog-fish (*Scyllium canicula*); in passerine birds; and in the Mammal.

Their largest development, however, is in the "Holocephalous" fishes—*Chimæra* and *Callorhynchus*.

These three pairs of rudimentary pre-oral arches are like the first post-oral of the Mammal, non-segmented; they are sometimes direct outgrowths of, and at others are formed separately from, the basal bar (trabecular).

I cannot see that these cartilages are anything else than arrested representatives of the large, fully developed post-oral arches; they correspond, however, only with the upper or suspensorial segment.

The great difference between the head and body in existing Vertebrates is shown in every part composing these two regions.

Therefore, any impatient premature attempt to make a perfect harmony between the parts that form the axial, the neural, and the hæmal regions of each, will end in disappointment.

Embryology must show us how true is the deep, essential, primary homology of these parts; but *morphology* must come in and demonstrate the great and inherited differences, slowly arising, no doubt, that are to be seen between the two regions.

There is a real *generic* likeness between the axis, the upper and lower arches, and the overlying parts in the cranial and spinal regions; let these be computed at their true worth in any comparison of the two categories one with another.

Thus, the *divided* basi-neural regions of the body are comparable with the *continuous* basi-neural regions of the head, and the visceral arches may be likened to ribs.

The "uncinate processes" of the ribs of certain reptiles (*Hatteria* and the Crocodilia), and in all birds, except *Palamedea*, are comparable to the branchial "rays" of Selachians; and the overlying "*extra-branchials*" are not void of a true similitude to the *girdles* of

the outgrowing limbs. I suspect, indeed, that they are their true serial homologues.

In this latter case, the splitting of the mesoblasts into several strata in the throat, on the one hand, and in the thoracic and pelvic regions of the body, on the other, are strictly comparable morphological changes.

I strongly suspect that if we could bridge over the gulf between the lancelet and the lamprey, we should find in these "connecting links" that the head was not a mere repetition of the body.

Somatomes we should find; but the intercalary skeletal parts would be found, I believe, to run into each other from the first, and the basi-neural cartilages of the head might be seen, *in time*, before the distinct neural arches of the spine.

I must now recapitulate a little.

After carefully considering the views and studying the researches of Huxley, Gegenbaur, Balfour, and Milnes Marshall, I am satisfied that, in spite of the doubling up of the basis cranii, at the time of its greatest flexure, there are rudiments of *three pre-oral arches*, related to *two pre-oral clefts*, namely, the lacrymal and the nasal.

That the mouth is caused by the blending together of a right and left cleft is the view held (I find) by Dr. Allen Thomson; this view, also, is held by Dr. Dohrn. (*See* "Balfour's Elasmobranchs," p. 15.)

The horseshoe fold of the mid-brain, the formation of the large hollow bed for the eyeball, and the special function to which the true segmental *motor oculi* nerve is confined, are all correlates of the special development of those wild branches of the trigeminal and facial nerves, namely, the ophthalmic and Vidian.

This is tantamount to saying that the hypertrophy of the first and second vesicles of the brain, and the large size of the optic vesicles which are outgrowths from the first of these, with all the enfoldings and complications necessary to complete the eyeball have, together, dominated all the surrounding parts, causing them to do many strange things, so to speak, vicariously.

Milnes Marshall finds that the olfactory nerves are solid until the seventh day of incubation in the chick; in embryos of the green turtle of the size of a horse-bean, I find the nerves still solid.

When the embryos are two or three times that size, these nerves each acquire a large cavity proximally, from the fore-wall of which the branches seem to spring.

The foremost of these branches spring from the top of the vesicle; they arose at first from the top of the fore-brain.

Both in the chick and embryo of the turtle, the fourth nerve, as soon as it can be found, runs a course so directly athwart the first branch of the fifth as to suggest its *non-segmental* nature.

The sixth nerve, or *abducens*, certainly arises from the ventral surface of the hind-brain; this being so, it manifestly corresponds to the anterior root of a spinal nerve; and as Milnes Marshall suggests, it may belong to the trigeminal, to the facial, or to both of these nerves.

If low forms should turn up, in which the optic nerves were truly segmental, and not direct vesicular outgrowths of the fore-brain, that would only affect the classification here-suggested, by showing that our present Vertebrata have lost a segment through the extreme specialization of the optic nerves.

As matters stand at present, we have, then, the nasal, lacrymal, oral, tympanic, and branchial clefts; of these we see that there are three in front of the tympanic, and there may be eight behind it.

Thus we get four pre-auditory and eight post-auditory clefts, with their nerves; if we add the twelfth (hypoglossal) of the "Amniota," we have obtained signs and proofs of thirteen cranial (segmental) nerves, all of these, except the last, forking over visceral clefts, and hedged in, all but the last, by visceral bars.

The first of the bars is in front of the first or nasal cleft, the last, or thirteenth, is the hinder bar of the lamprey's branchial basket work.

Of course in this classification I do not mention, for the time, the distinction between the *deep* and *superficial* cartilages.

In the lowest kind of chondro-cranium known to us, namely, that of the sucking-fish and the larvæ of the Batrachia, the first post-oral arch is not only very largely developed, but is also carried forwards directly to the front of the head; it does not of itself form the skeleton of the oral opening, but carries the large cartilages that form the peculiar suctorial mouth.

Hence, in the lamprey and in the Batrachia, whilst they are in the larval condition, the pre-oral visceral bars are arrested in their growth; in the adult of the latter types, when the permanent mouth is formed, then two (of the three possible) visceral arches are developed.

Not only in low forms are the anterior visceral arches arrested, or even suppressed, but the visceral clefts also, in front of the mouth as well as the one immediately behind it, are often imperfectly developed, or even suppressed.

The nasal cleft does not remain open inwards until we get to the Dipnoi and Amphibia, and the lacrymal cleft not until we get to the Amniota.

No one has seen a first post-oral or tympanic cleft in the lamprey, and its second post-oral, discovered a few years ago by Professor Huxley ("Proc. Roy. Soc.," vol. xxiii, p. 129), in the larva, or *Ammocœte*, is very small, and apparently has no external opening.

In all the Urodeles, and in the lower and more generalized Anura,

the first post-oral or tympanic cleft is either suppressed or forms a very slight *inner pouch;* it never opens externally in the Anura.

In most of the Ganoids, as in the Selachians, the first post-oral cleft persists as the spiracle, but in osseous fishes it is a very temporary structure.

I have long suspected that the archaic (entomocranial) Vertebrata were often supplied with a perfect circlet of branchial filaments around their mouth.

We know the first post-oral cleft to be a branchial cleft with several branchial filaments; Dr. Milnes Marshall's researches show that the nasal folds of the embryo Selachian are developed in precisely the same manner as the external branchials of the spiracular opening.

These filaments become converted into a pseudo-branchia; the natural suggestion is that the nasal folds, having the same structure, and being formed in a homologous space, are indeed nothing more, even in the adult, than a modified pseudo-branchia.

Further Remarks on the basi-neural Plates of the Skull.

I have latterly come back to the same view as to the meaning of the "trabeculæ cranii," as was first propounded by Rathke, namely, that they are there extensions forward of the basal plates or "investing mass."

Professor Huxley's term "para-chordal" is as applicable to the hind half of the trabeculæ, in embryos of Selachians and Amphibians, as to the pair of plates behind the trabeculæ, or "para-chordals" proper.

Moreover, in several kinds of Urodeles, namely, (*Spelerpes, Desmognathus, &c.,*) there is a *hindmost* pair of para-chordal cartilages in front of the *functionally first* vertebra.

It behoves us, therefore, not to be led astray by mere words; I should prefer to call the paired cartilages of the notochordal region of the head *basi-neural;* this term would satisfy not only some of the best authors, such as Goette and Balfour, but it also expresses what I conceive to be the actual nature of the parts.

These right and left tracts of cartilage that form the main part of the base or skull floor, and grow, more or less, over the overgrown neural axis, are divisible into two regions, the hind region, which is *para*-chordal, and the fore-part, which is *pro*-chordal.

These essentially continuous tracts were, I suppose, developed, first, as a support to the increasing fore end of the neural axis, and from the first were not clearly divided into intercalary (inter-neural) tracts; and moreover, as I take it, they solidified earlier than the inter-neural tracts of the spinal region.

That *these latter,* even, were not always from the first developed

into separate tracts the existing *skates* and *rays* show; as the tissue became more and more solid, it became, in relation to the action of the muscle-plates, differentiated into a paired chain of pieces of solid cartilage for the *origin* and *insertion* of the segmental muscles.

The "basi-neural" tracts of the head in existing Vertebrata are developed very differently in different types, some parts forestalling others in chondrification, and yet not similarly in all.

In the Salachians (*Scyllium*, *Raia*, *Pristiurus*, see "Trans. Zool. Soc.," vol. i, Part IV, Plates 33–42) these tracts are very broad, and there are *three* regions made evident by the earlier or later time at which they become converted into hyaline cartilage.

The middle region chondrifies first; this extends from the nasal sacs to the middle of the auditory capsules, and answers to the "trabeculæ" without their "cornua."

The next tract is behind the trabeculæ; it does not end close behind the exit of the *vagus*, but runs on for a considerable distance into the spine, without any sign of segmentation.

The last to solidify its *embryonic*, into *hyaline*, cartilage, is the *inter-nasal;* this tract is, at an early stage, narrow, for the nasal sacs are immense; afterwards these structures get much further apart, and allow of the free development of the intervening cartilage.

This overshadowed, pinched tract of the skull, breaks out into three parts in front of the nasal sacs,—a pair of bilobatea, "cornua trabeculæ," and the azygous "pre-nasal" rod.

That rod is the fore part of the *inter-trabecular tract;* it is but little evident between the nasal roofs, and between the eyes only forms the thinner middle part of the cranial floor.

But the azygous cartilage, behind the pituitary body, forms the undivided, secondary (mesoblastic) sheath of the notochord; in the early embryos it runs on from the pituitary space to the end of the body: afterwards, it is segmented at the occiput, and behind that region becomes segmented (more or less) into "centra."

Yet in the rays a large *post-occipital* tract is continuous, permanently; and in both rays and sharks it shows at first *signs of extensive segmentation in the head,* for the notochord at its fore end becomes "moniliform." I find *five* joints or beadings in *Pristiurus*, and *eight* in *Scyllium*.

In the Urodeles ("Skull of Urodeles," Plate 22, fig. 1, *tr*), for instance in the axolotl-embryos at the time of hatching (my *second* stage), show a pair of broad para-chordal cartilages that embrace the fore half of the relatively huge notochord. These grow in front ot that rod as small rounded horns, embracing the sides of the fore-brain below.

Afterwards these rods grow up to, and then between, the nasal cap-

sules, and then spread into dilated out-turned "cornua." By that time another pair of para-chordal cartilages has appeared behind the first, and in several species of Urodeles a much smaller pair behind them. I do not find more than a very delicate layer of mesoblast sheathing, the cephalic part of the notochord; and the inter-trabecular cartilage in front of it merely fills the interspace of the trabeculæ in the nasal region; only in *Siren* and *Salamandra* do I see a short "pre-nasal" rod.

In most of these types the two last-mentioned being exceptional, the para-chordal part of the trabeculæ is absorbed, and only a selvege of the para-chordal, proper, remains inside the ear-capsules, and at the occipital condyle.

In these types the basal cartilage does not grow up into the cavity of the mid-brain as a "post-clinoid" wall, but this wall is very well seen in the dog-fish (*Scyllium*).

In the Batrachia ("Anura") the trabeculæ embrace the apex of the notochord less, and develop their cornua sooner; the rest of the basal plate, behind, is somewhat later in its appearance. They have a continuous growth of the *inter-trabecular* band, which is a flat floor finishing the skull below; between the eyes, and between the nasal sacs it grows into a wall; in front, this tract fills in the angle between the trabecular cornua, and often (as in the tree-frogs) sends forth a distinct pre-nasal rod, like that of the sharks.

The Anura have a slight post-clinoid wall; their skull on the whole, *when finished*, is intermediate between that of the Selachians and the Urodeles. Goette mentions the cartilaginous sheath of the cranial notochord in *Bombinator;* I find it very massive in the larvæ of *Pseudis* (when the limbs are just appearing); but, as a rule, this sheath is very thin in tadpoles.

In Teleostei ("Salmon's Skull," Plates 1—8) the investing mass chondrifies first, and is relatively much larger than the trabeculæ: they are only *sub-distinct*, and soon coalesce; the trabeculæ grow very rapidly, so that at the time of hatching there is a very massive *pro*-chordal tract.

By the middle of the second week after hatching the *pro*-chordal tracts are quite distinct from the *para*-chordal, and lie on them obliquely.

By the middle of the first summer the tracts show, as far as I can see, no sign of their separation; but in the adult salmon the trabeculæ only reach to the front of the pituitary space, whilst the anterior half of the para-chordal cartilage receives its bony matter from their "prootics."

In the salmon the middle ethnoid is at first feebly developed, growing down, as a keel, from the "tegmen cranii;" and in the adult all we see of the *inter-trabecular* point is a short wall between the lateral

ethnoidal masses; for, further forwards, at the mid-line, there is a large cavity filled with fat.* In front the trabeculæ end in two short cornua, but there is no "pro-nasal" rod.

In the snake ("Snake's Skull," Plates 27—33) the cranial part of the notochord shortens rapidly, and is invested with a very thin secondary sheath. The trabeculæ are manifestly merely *fore-growths* of the para-chordal tracts: there is no inter-trabecular tract, and even the "septum-nasi" is formed by the two trabecular crests uniting with the inner edge of each nasal roof. The snake has a post-pituitary wall, which is well developed in all the "Sauropsida."

In the high-skulled lizards the inter-trabecular cartilage appears, and largely contributes to the formation of the partition wall in the fore part of the head; it does not, however, appear, wedging in between the trabeculæ below, as in the Chelonians, nor has it any *pre-nasal* growth in any kinds I have worked out.

But the Chelonians shed most light upon these parts, and my recent work at this type has made many things clear to me that for a long time have remained unexplained.

The segmentation of the pro-chordal from the para-chordal region is both *secondary* and *temporary:* it did not exist in my *first* stage, and had vanished in the *fourth*, namely, in embryos three-parts ripe; these, which were the size of a horse-bean (*third* stage), corresponded with what is seen in the adult salmon.

The embryos of *Struthio camelus* come very close to *Chelone* as to the inwedged position of the inter-trabecular tract, which appears below, and makes the base of the orbital septum carinate.

This is not seen in the other "*Ratitæ*" nor in the "Carinatæ;" but I find it in the ethnoidal region in the chick ("Fowl's Skull," Plate 81).

Also in having large orbito-sphenoids the African Ostrich comes nearest to *Chelone.* Neither of these types, nor the other "Ratitæ," show the segmentation of the perpendicular ethnoid from the septum nasi, which is constant in the "Carinatæ," and is seen to begin in Lizards.

In Mammals (Pig's Skull, Plate 28, fig. 8), the para-chordals chondrify before the pro-chordals; the latter, however, are never separated from the former. The "inter-trabecular" wall dominates in the front of the pituitary space, but the thin, flattened, vertical trabeculæ diverge at a certain point to form the huge orbitosphenoids, exactly as in *Chelone* (Plate 33, fig. 6).

The shortening of the skull by the fold of the mid-brain, and the high post-clinoid wall growing up into the hollow, are just alike in the turtle and the pig.

* Dr. Milnes Marshall has shown me that the want of symmetry seen in my salmon embryos was artificially produced by the spirit; they should have been re-*moved from the egg* before they were preserved.

On the Organic and Actual Fore End of the Head.

When the cephalic fold of the embryo is formed, the fast-growing cerebral bulbs hang over the yolk; the brain at this stage, by its own bulk and weight, hangs down like a gourd.

Of necessity, this throws together parts that would be at some considerable distance from each other; also the *organic* end is not the *actual* end of the head, as I have mentioned before: *that* is formed by the mid-brain; the fore-brain, or terminal vesicle, looks downwards and backwards.

In this piece of morphology, as in studying the "receptacle of the fig-tree," we have to distinguish between the *apparent* apex and the *true* apex.

But if organs have to be supported, the morphological force must make good that which is thrown out of gear by this special hypertrophy of parts, and the beams and rafters must be eked out.

For this heavy nodding bend of the growing brain so doubles up the undergrowths of the skeletal rudiments, that both the paired and unpaired parts are stopped very near to the *retral* organic apex.

Also the nasal sacs and optic vesicles, attached to, or growing from, the fore-brain, are in a new position, and not, as they should be, in the old straight line.

Indeed, the axis of these skeletons grows up far into the hollow bend of the mid-brain, and the notochord turns over somewhat, and then loses itself in the mesoblastic sheath, which gets very close to the *organic end* of the brain.

The paired tracts (para-chordals, both trabecular and post-trabecular) grow up into that hollow, and there stop; their new outgrowths (or eking out of the basi-neural tracts) begin again at the base of this ascending wall.

Also the middle (*unpaired*) part begins again directly, in *Chelone*, and in embryos (two-thirds ripe), in which the notochordal sheath was ossifying to form the beginning of the "basi-occipital" and "basi-sphenoid," the *cephalostyle* passed at once from the notochordal sheath to embrace and ossify the "inter-trabecular" bar, close below the pituitary body.

That part of the basi-sphenoid which lies in front of and below the pituitary body is formed by ossification, directly from the cephalostyle, of the unpaired solid pro-chordal tract of mesoblast; afterwards the bony matter spreads into the paired bars (trabeculæ).

But that middle bar came short of the pituitary space in the earlier stages; for there the *oral fold* grew up to graft itself upon the fore-brain, close behind (or *below*, organically), the true apex or end of the brain.

Hence we see, that if the pituitary body were formed in an unbent

embryo, it would be close below, and only a little behind, the fore end of the creature; in this supposed type the notochord would be only a little behind the *punctum terminale*.

Now the fore and mid brain have at present only yielded to embryologists *one pair each* of segmental nerves growing from their dorsal region; the hind brain is a *series* of enlargements.

The two great *pre-aural* segmental nerves (5th and 7th) by the overfolding of the brain, are enabled to send on to the front of the head their special branches, needed there, because of the specialization (for motion) of the *third*, and the specialization (for sensation) of the *first* nerve.

Thus these three-branched nerves have grown in harmony with the paired and unpaired *basi-neural* cartilages, and there is a due extension forward of cartilages to the partially straightened skull, and a due supply of nerves from behind.

But in spite of all the metamorphoses of these parts, neural and skeletal, if Dr. Milnes Marshall's observations (with which mine accurately accord) be true, then we still have two true segments in point of the cleft (*oral*) which is forked over by the 5th nerve. It could not be expected that the visceral arches and intervening clefts would be otherwise than greatly modified and masked in the fore part of the head, with its huge nervous centres, and highly complex organs of special sense.

The larvæ of the Amphibia, especially of the "Anura," have been very carefully studied by me, as likely to throw light upon the order of development of the cranial-facial skeleton; the lamprey, also representing those larvæ permanently, has been the subject of much thought, as a sort of practical pattern of those larvæ. In these forms the *extra-visceral* skeleton of the head is much developed, and only part of the true visceral (internal arches) appear.

For the mouth in these forms is terminal, and its skeleton is made up of *sub-cutaneous cartilages*, the serial homologues of the subcutaneous basket-work of the large respiratory pharynx of the lamprey.

In that form the only true visceral arches developed are the mandibular and the hyoid; a basal rudiment of the internal branchial arches exists as the "lingual cartilage."

The free mandible of the lamprey is packed up, and apparently functionless, close behind the postero-superior "*labial;*" the quadrate portion of the "suspensorium," is a mere point or style, with no condyle.

This suspensorium throws a fold of cartilage over the second branch of the 5th nerve and the temporal muscle; this is not the pterygoid cartilage, and is only seen in this type, in tadpoles, and in some chondrosteous Ganoids, *e.g.*, *Planirostra*, as shown by Mr. Bridge.

Also the epi-hyal is in a low state of development; there is a cerato-hyal and a basi-hyal piece, growing forward below, in front of the large lingual ("basi-branchial") cartilage.

But there is a copious growth of external cartilage both around the terminal mouth and around the huge branchial pharynx; the cranial box is at a low state of development, and the fore part of the head shows no trace of a pre-oral arch.

In tadpoles we have a very similar state of things, but there is a real ascent; the suspensorium develops a quadrate condyle, and on this the *passive* mandible is hinged.

Round the mouth, cartilages quite like those of the lamprey, are developed, but they are smaller; and there are only four bars (pouches) in the walls of the pharynx; rudiments of four *true intra-branchials* also, are developed. A fifth subcutaneous cartilage appears during metamorphosis, belonging to the mandibular arch; it becomes the cartilaginous "annulus tympanicus."

After it has appeared the "styloid cartilage" of the lamprey ("epi-hyal") is, in them, slowly developed, and becomes the "columella auris."

Also, during metamorphosis, the rudiment of a "palatine" visceral arch appears, and in the genus *Bufo* becomes a large distinct *pre-oral* cartilage. After metamorphosis, another cartilage appears on each side, within the nasal cleft, the "pro-rhinal."

My idea of the order, *in time*, of the skeletal elements is as follows:—

First. The superficial cartilages of the mouth and respiratory pharynx.

Secondly. Basi-neural, and then, afterwards, going from them, *visceral* cartilages, in the *inner layer* of the walls of the mouth and throat.

Thirdly. After that, selection of dermal scutes, first as scales and afterwards as splint-bones ("parostoses"), to supplement, for supporting purposes, the chondro-cranium.

Fourthly. A gradual *arrest*, and then more or less of *suppression*, of the chondro-cranial parts, and the increased use of subcutaneous investing bones, at times in conjunction with remnants of the *old primary superficial* cartilages.

The development of the spine has been, I believe, a thing of *later date;* and the limb-girdles and limbs newest and latest of all.

The brain, mouth, and throat, with coiled intestines, whose outlet is very little behind the occiput, make up all that is of any consequence, in such a form as the gigantic tadpole of the paradoxical frog (*Pseudis*); whose post-cranial segments have evidently been super-additions, developed for the sake of locomotion—to form a mere swimming organ.

Behind the head, the segments for free motion cannot be moved by the developing segmental muscles until an intercalary segmentation has taken place; hence the vertebral segments which come between the "muscle-plates" and spinal nerves.

The head, eschewing such mobility, has developed an axial box for the brain, and beneath this firm structure, the mobile and distensible mouth and throat are swung.

III. "On an Extension of the Phenomena discovered by Dr. Kerr and described by him under the title of 'A New Relation between Electricity and Light.'" By J. E. H. GORDON, B.A., Assistant Sec. of the British Association. Communicated by Professor TYNDALL, F.R.S. Received February 10, 1879.

In November, 1875, Dr. Kerr announced in the "Philosophical Magazine," that he had discovered a new relation between electricity and light. He showed that when glass is subjected to an intense electrostatic stress, that a strain is produced which causes the glass to act like a crystal upon polarized light.

On Wednesday, February 5, 1879, I was working at this experiment in the Royal Institution, and endeavouring, by means of the electric light, to project the effect on a screen, in preparation for a lecture on the next day.

In the experiment as described by Dr. Kerr, and which was shown plainly on the screen, on February 6, the light is extinguished by the Nicols, and reappears when the coil is set going.

In the projection experiment a patch of moderately bright white light, about 3 inches diameter, appeared on the screen when the coil was worked. The images of the points inside the glass were about 1½ inches apart. On Wednesday, however, the electrostatic stress was accidentally allowed to become strong enough to perforate the glass. Immediately before perforation there occurred the effects which are the subject of the present communication.

First appeared a patch of orange-brown light about 6 or 7 inches diameter. This at once resolved itself into a series of four or five irregular concentric rings dark and orange-brown, the outer one being perhaps 14 inches diameter. In about two seconds more these vanished and were succeeded by a huge black cross about 3 feet across, seen on a faintly luminous ground. The arms of the cross were along the planes of polarization, and therefore (the experiment being arranged *according to* Dr. Kerr's directions) were at 45° to the line of stress.

The glass then gave way, and all the phenomena disappeared except the extreme ends of the cross, and the discharge through the hole, where the glass had been perforated, was alone seen.

The phenomena were seen by Mr. Cottrell, by Mr. Valter (the second assistant), and by myself. A fresh glass plate was at once drilled in hopes of repeating the phenomena in the lecture next day, but owing to sparks springing round we did not succeed in perforating the glass, and therefore saw only the faint return of light described by Dr. Kerr.

Some more glasses have been prepared and their terminals insulated, and I now propose to make another attempt to repeat the new effects before the Royal Society.

February 20, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On Electrical Insulation in High Vacua." By WILLIAM CROOKES, F.R.S. Received February 6, 1879.

The experiments here described were tried nearly two years ago. They were suggested by some observations I was then making on the passage of an induction current through highly exhausted tubes. The main branch of the research being likely to occupy my attention for some time, I may be unable to return to these less important offshoots. I have ventured, therefore, to embody them in a short note for the "Proceedings of the Royal Society."

A pair of gold leaves were mounted, as for an electroscope, in a bulb blown from English lead glass tubing. The leaves were attached to a glass stem and the lower part of the bulb was drawn out for sealing to a Sprengel pump as shown at fig. 1. A stick of ebonite excited by friction was generally used as the source of electricity, but any other source will do equally well, provided it is not too powerful.

No special attention was paid to the action of electricity on the leaves in air or at moderate vacua, as it agreed with what is already well known. The exhaustion was pushed to a very high degree (about the millionth of an atmosphere), when it was found that the excited

ebonite had a much greater effect on the gold leaves than at a lower exhaustion; for a long time however I was not able to charge the leaves permanently, in consequence of their falling together as soon as the source of electricity was removed.

FIG. 1.

When a hot substance was brought near the bulb facing a gold leaf, so as to warm the glass, molecular repulsion took place, and the leaves retreated from the warm spot, standing out at an angle of about 45°. As the glass cooled the leaves resumed their former vertical position.

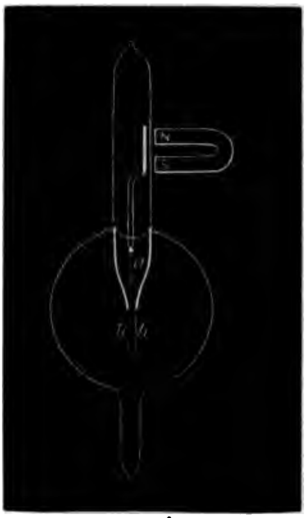
While the leaves were repelled from the hot glass, the excited ebonite had a very powerful action on them, and if it were brought near hastily, the leaves flew off to the side of the glass, destroying the apparatus. By careful management and repeated trials, however, the ebonite could be brought near the warm spot of glass, the leaves suddenly extending at an angle to each other. The appearance was as if a spark had been able to pass across the bridge formed by the line of advancing and retreating molecules connecting the hot glass with the gold leaves. On the ebonite being removed and the glass allowed to cool, it was found that the repulsion of the leaves was permanent. The rubbed ebonite would attract and repel them as it was moved to and fro, but the angle formed by the leaves with one another remained unchanged. A warm body brought near the glass opposite one leaf would repel the pair as a whole; on then warming the opposite side of the glass repulsion on that side took place, the angle of the leaves being somewhat diminished, but on cooling the leaves opened again to their former extent.

When the glass bulb was strongly heated by a spirit flame the leaves suddenly discharged and fell together.

Another bulb (fig. 2) was prepared, containing a plate of mica, *a*, *which could* be suddenly placed between the gold leaves, *bb*. The

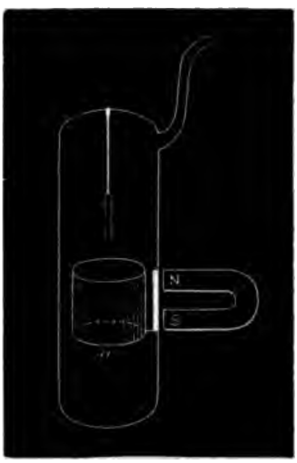
plate of mica was longer and wider than the gold leaves, and was connected with a small piece of iron wire, capable of moving up and

FIG. 2.



down a tube sealed into the top of the bulb. By means of an outside magnet the mica plate could thus be lowered between the gold leaves or raised out of their way, as desired. The tube was exhausted to about the millionth of an atmosphere, the mica plate being held quite above the leaves. One side of the bulb was then heated, and the

FIG. 3.



leaves permanently charged by means of the excited ebonite. The mica plate was now carefully lowered. As it came between the gold leaves they diverged further apart, and kept so as long as the mica plate was between them. On removing the plate the leaves reassumed their former divergence. This could be repeated any number of times.

A similar piece of apparatus (fig. 3) was made, only instead of a mica plate coming between the leaves, a mica cylinder, *a*, capable of being raised and lowered outside the divergent leaves, was employed. I was not able to get entirely concordant results with this, owing to the friction of the mica developing electricity on the inner surface of the glass tube; but in all cases, when the cylinder was raised until it covered the electrified leaves, it had the effect of diminishing the angle which they formed with each other.

The following experiments were also tried:—the leaves being separated about 160°, as at fig. 4, A, one side of the tube was slightly

FIG. 4.

heated by a spirit flame. The leaf on that side fell to a vertical position, and remained so when all was cold, the other leaf sticking out as before, as at B. This would seem to show that the divergence of the leaves in this case was not so much due to their mutual repulsion, as to an attraction exerted on each of them by the inner surface of the glass tube. The remaining divergent leaf could be slightly lowered when the glass tube above it was warmed with a bunch of cotton wool dipped in hot water. On cooling the leaf rose again to its original position. When this side of the tube was also heated with a lamp, the leaf was repelled down, but not so readily as the other had been, and when the tube got cold, it rose to nearly its former position. This was repeated several times with uniform results. When the leaf was repelled down, the vertical leaf also

moved away, so as to keep the same angle between them. It is therefore evident that the leaves themselves were also charged.

FIG. 4.

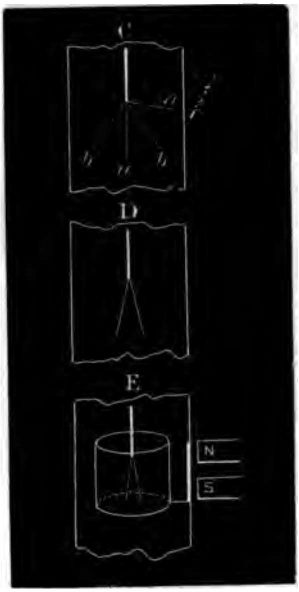


Fig. 4, C, shows the two positions of the leaves, *aa* before applying heat to the side *c* of the tube, and *bb* after heating the glass at *c*.

The tube was now heated on both sides, causing the leaves to come nearer together as shown at fig. 4, D. While the glass was warm the cylinder was raised so that it surrounded the leaves: this caused them to get a little closer together, and they kept in this position, shown at E, after the whole apparatus was quite cold.

After remaining thus for some time, the cylinder was lowered, and the leaves widened out and took up the position shown at *bb*, fig. 4, C. They did not return to the position *aa*, showing that their divergence was now owing to their own mutual repulsion, and not to an attraction of one or other to the electrified glass.

In December, 1877, I totally immersed one of these exhausted glass bulbs in a vessel of water; the gold leaves having previously been charged, and standing at an angle of 112° from one another, as at fig. 5. The water was connected electrically with "earth," and the whole was set aside in a cabinet on the 1st of January, 1878.

At the present time, after having remained in this condition for thirteen months, the leaves form exactly the same angle with one another which they did when they were first put in the cabinet.

FIG. 5.

From this experience I think we may consider that at an exhaustion of a millionth of an atmosphere, air is an absolute non-conductor of statical electricity. It is, therefore, legitimate to conclude that the vacuum of interstellar space offers equal obstruction to the discharge of electrified bodies, without necessarily interfering with their mutual repulsion if similarly electrified. It is possible that in these facts an explanation may be found of some obscure celestial phenomena.

II. "On the Reversal of the Lines of Metallic Vapours." No. IV. By G. D. LIVEING, M.A., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received February 12, 1879.

In the experiments described in the following communication, instead of introducing the substances to be observed in the metallic form into our tubes, we have endeavoured to overcome, to some extent, the difficulty of the presence of impurities by making use of reactions which should generate the metallic vapours within the tubes. For this purpose we have generally employed the great reducing power of carbon and of aluminium at high temperatures.

In a former communication ("Proc. Roy. Soc.," vol. xxvii) we described the reversal of the two blue lines of cæsium and the two violet lines of rubidium by the vapours of those metals, produced by heating their chlorides with sodium in glass tubes. It might be doubtful from these experiments whether the absorption were due to the metals or to the chlorides. To decide this question, we first tried cæsium chloride by itself, heated in a tube such as we used before. No absorption lines could be seen, although a good deal of the chloride had been vaporized and distilled to the cool part of the tube. The *experiments were* next repeated, both with rubidium and cæsium

chlorides along with metallic lithium. The two violet lines of rubidium and the two blue lines of cæsium were reversed, as when sodium was used instead of lithium, and as the lithium gave no sensible vapour, the observations could easily be continued for a much longer time with the same tubes. No other absorption lines could be discerned. It may be observed, however, that it is not easy to obtain a source of light sufficiently rich in the least refrangible red to allow of observations on the absorption of light so little refrangible as the red rubidium lines. A platinum wire, heated nearly to fusion by an electric current, appeared to give the brightest light in this part of the spectrum, but of that light no definite absorption by the rubidium could be observed in the red. We then had some mixtures of carbonate of cæsium with carbon, and of carbonate of rubidium with carbon, prepared by charring the tartrates; and observed the results of heating these mixtures in narrow porcelain tubes, placed vertically in a furnace, as described in our first communication on this subject ("Proc. Roy. Soc.," vol. xxvii). A small quantity of the cæsium mixture, introduced into a tube at a bright red heat, showed instantly the two blue lines reversed and so much expanded as to be almost in contact. The width of the dark lines decreased as the cæsium evaporated, but they remained quite distinct for a very long time. A similar effect was produced by the rubidium mixture, only it was necessary to have the tube very much hotter, in order to get enough of violet light to see the reversal of the rubidium lines. In this case the two lines were so much expanded as to form one broad dark band, which gradually resolved itself into two as the rubidium evaporated. The reversal of these lines of cæsium and rubidium seems to take place almost or quite as readily as that of the D lines by sodium, and the vapours of those metals must be extremely opaque to the light of the refrangibility absorbed, for the absorption was conspicuous when only very minute quantities of the metals were present. The red, yellow, and green parts of the spectrum were carefully searched for absorption lines, but none due to cæsium or rubidium could be detected in any case. It is perhaps worthy of remark that the liberation of such extremely electro-positive elements as cæsium and rubidium from their chlorides by sodium and by lithium, though it is probably only partial, is a proof, if proof were wanting, that so-called chemical affinity only takes a part in determining the grouping of the elements in such mixtures; and it is probable that the equilibrium arrived at in any such case is a dynamical or mobile equilibrium, continually varying with change of temperature.

Our next experiments were with charred cream of tartar in iron tubes, arranged as before. In this case a broad absorption band appeared, extending over the space from about wave-length 5,700 to 5,775, and in some cases still wider, with edges ill-defined, especially the more refrangible edge. By placing the charred cream of tartar in

the tube before it was introduced into the furnace, and watching the increase of light as the tube got hot, this band was at first seen bright on a less bright background, it gradually faded, and then came out again reversed, and remained so. No very high temperature was required for this, but a rise of temperature had the effect of widening the band. Besides this absorption, there appeared a very indefinite faint absorption in the red, with the centre at a wave-length of about 6,100, and a dark band, with a tolerably well-defined edge on the less refrangible side, at about a wave-length of 4,850, shading away towards the violet. A fainter dark band was sometimes seen beyond, with a wave-length of about 4,645; but sometimes the light seemed abruptly terminated at about wave-length 4,850. It will be noticed that these absorptions are not the same as those seen when potassium is heated in hydrogen, nor do they correspond with known emission lines of potassium, although the first, which is also the most conspicuous and regularly visible of these absorptions, is very near a group of three bright lines of potassium. It seemed probable that they might be due to a combination of potassium with carbonic oxide. We tried the effect of heating potassium in carbonic oxide in glass tubes, but, though the potassium united readily with the gas, the compound did not appear to volatilize at a dull red heat, and no absorption, not even that which potassium gives when heated in nitrogen under similar circumstances, could be seen. We then tried induction sparks between an electrode of potassium and one of platinum in an atmosphere of carbonic oxide. The usual bright lines of potassium were seen, and also a bright band, identical in position with the above-mentioned band, between wave-lengths about 5,700 and 5,775. This band could not be seen when hydrogen was substituted for carbonic oxide. A mixture of sodium carbonate and charred sugar, heated in an iron tube, gave only the same absorption as sodium in hydrogen. There were also no indications of any absorption due to a compound of rubidium or of cæsium with carbonic oxide.

The experiments of Mallet ("Chem. Soc. J.," 1876) on the volatility of calcium, strontium, and barium, and the reducing action of aluminium on the oxides, especially in the presence of carbonate of sodium, induced us to try similar mixtures in our tubes.

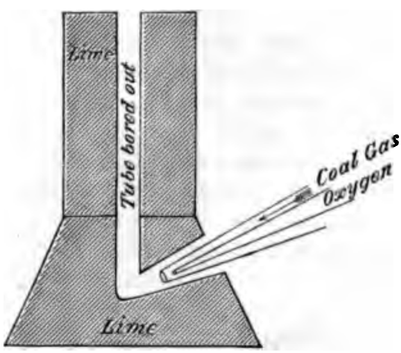
A mixture of barium carbonate, aluminium filings, and lamp-black, heated in a porcelain tube, gave two absorption lines in the green, corresponding in position to bright lines seen when sparks are taken from a solution of barium chloride, at wave-lengths 5,242 and 5,136, marked α and β by Lecoq de Boisbaudran. These two absorptions were very persistent, and were produced on several occasions. A third absorption line, corresponding to line δ of Boisbaudran, was sometimes seen, and on one occasion, when the temperature was as high as could be obtained in the furnace fed with Welsh coal, and a

mixture of charred barium tartrate with aluminium was used, a fourth dark line was seen with wave-length 5,535. This line was very fine and sharply defined, whereas the other three lines were ill-defined at the edges; it is, moreover, the only one of the four which corresponds to a bright line of metallic barium.

Repeated experiments with charred tartrates of calcium and of strontium mixed with aluminium gave no results, but on one occasion, when some sodium carbonate was used along with the charred tartrate of strontium and aluminium, the blue line of strontium was seen reversed, and on another occasion, when a mixture of charred potassium, calcium, and strontium tartrates, and aluminium was used, the calcium line, with wave-length 4,226, was seen reversed. The fire in this case was fed with gas retort carbon, and the temperature such that iron tubes, though well coated with fire-clay, gave way in a few minutes. It appears, therefore, that the blue line of strontium, and the above-mentioned violet line of calcium, are reversible by this method, but not so easily or so certainly as the lines of barium or its compounds above mentioned.

In order to obtain higher temperatures than we could obtain in the furnace used in our former experiments, we have made preliminary experiments with lime crucibles heated (1) by a jet of coal-gas and oxygen; (2) by the electric arc. For this purpose a block of chalk or lime had a vertical tubular hole bored into it about 6 or 7 millims. in diameter, and for the gas jet a second lateral boring, meeting the other boring at the bottom (fig. 1). For the electric arc two lateral

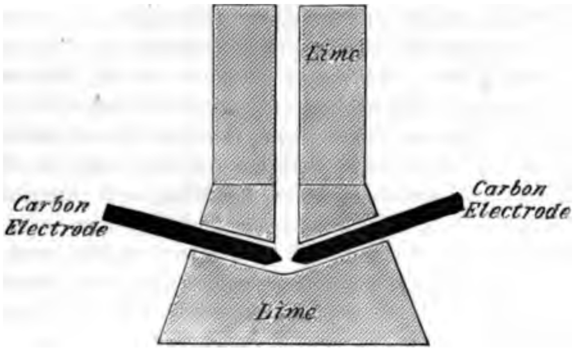
Fig. 1.



borings are made on opposite sides of the block, meeting the vertical boring at its bottom (fig. 2). Above the crucible we place a mirror inclined at 45°, so as to reflect the light from the vertical boring on to the slit of a spectroscope, a plate of mica being interposed between the mirror and crucible to deflect the stream of hot gas and

catch the smoke, which would otherwise soon dim the surface of the mirror.

FIG. 2.



With the jet of coal-gas and oxygen the usual green and orange bands of lime and the violet line of calcium (wave-length 4,226) were seen bright on the continuous spectrum, and on dropping in some aluminium their brightness increased at the same time that a dark line appeared in the middle of both the orange and green band. This dark line speedily disappeared in the case of the orange band, but lasted longer in the green band. When some lithium carbonate was put into the crucible and the coal-gas turned on so as to be in excess, the red lithium line was reversed, appearing slightly expanded with a black line down the middle.

For the electric arc 25 Grove's cells were used, and the carbon poles introduced through the lateral openings, so as to meet at the bottom of the vertical boring of the lime crucible. A very brilliant spectrum of bright lines was produced on bringing the poles in contact, while a copious stream of vapours ascended the tube. On drawing apart the poles, which could be done for nearly an inch without stopping the current, the calcium line (wave-length 4,226) was seen reversed. On dropping some aluminium into the crucible the calcium line just mentioned was very much expanded, and appeared with a broad black line in the middle. The other calcium lines in the neighbourhood on the less refrangible side were also expanded considerably, but were not seen reversed. The more refrangible lines (Fraunhofer's H), however, remained sharply defined, and did not appear sensibly expanded or reversed.

On introducing some strontia the blue strontium line (wave-length 4,607) was immediately seen reversed, appearing as a broadish bright band, with a dark line in the middle. There were no indications of the reversal of any other strontium line, though the more refrangible lines were conspicuously bright.

When lithium carbonate was introduced the red line was at once seen reversed, but not much expanded. When some aluminium was added, the lithium blue line (wave-length 4,604) was seen with a dark line in the middle for a short time only. The green line of lithium was very bright indeed, and appeared somewhat expanded on the addition of aluminium, but showed no reversal.

On putting some baryta into the crucible the line with wave-length 5,535 was reversed, appearing very black but narrow. No other barium line could be seen reversed in that crucible, but in another crucible into which magnesia had been introduced, a dark line, with wave-length about 4,930, was observed, which may probably be ascribed to barium.

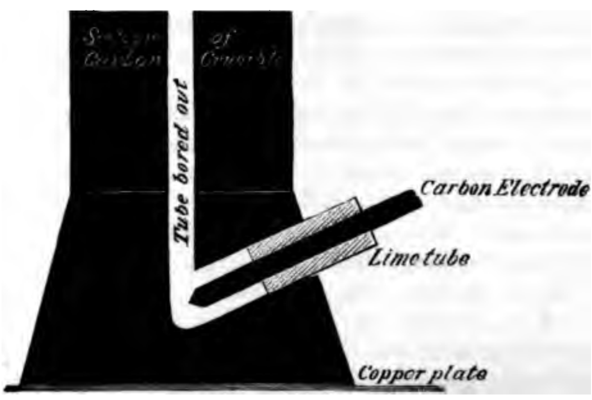
With magnesia and aluminium the least refrangible of the *b* group was seen reversed, all the *b* group being expanded.

When silver was introduced, on drawing the poles apart, both the brightest green lines (wave-lengths 5,464 and 5,209) were seen for a short time with a black line down the middle.

Frequently on parting the poles, whatever might be the substance in the crucible, the whole of the brightest part of the spectrum, from the orange to the blue, appeared filled with dark lines, all equidistant and equally dark, like a fine grating. With a high dispersion these lines are seen to be ill-defined at the edges. We can only suppose them to be a banded spectrum of some compound of carbon.

The lime crucibles are very quickly destroyed, but we hope to get some more compact lime than we have hitherto had, and to employ a more powerful electric current. The use of carbon or magnesia for crucibles will, we anticipate, enable experiments of this kind to be extended much further, and applied to various reactions taking place at the temperature of the arc. In the case of carbon crucibles the block of carbon itself will form one electrode, the other electrode

Fig. 3.



passing through a tube of lime as in (fig. 3). It is our intention to try a combination of the electric arc and induction spark in these crucibles. It is hardly necessary to note that the projection of the reversals of the lines of metallic vapours may be effected by this method better than by any method heretofore in use.

February 27, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Major-General Thuillier (elected 1869) was admitted into the Society.

The following Papers were read:—

I. "Studies in Acoustics. I. On the Synthetic Examination of Vowel Sounds." By WILLIAM HENRY PREECE and AUGUSTUS STROH. Communicated by the PRESIDENT. Received February 17, 1879.

[PLATES 6, 7.]

1. The authors of this paper have devoted much time during the past twelve months to a study of sonorous vibrations and the reproduction of speech. The invention of the phonograph has proved a great stimulus to this study. Many have worked in the same field, and many of the facts elicited by the authors have been anticipated by those who have been able to give more continuous study to the subject. Nevertheless, the mode of enquiry, the apparatus employed, and the results obtained are thought to be of sufficient novelty to justify their being brought before the Royal Society.

2. The curves traced by the vibrating disk of the phonograph on tinfoil, whether examined microscopically or reproduced by a species of pantelograph, were soon found to be insufficiently delicate to give the nicer shades of sound, and to fail to indicate the true curve of vibrations in all cases. This is shown by the imperfect reproduction of speech by the phonograph itself; the merging of the labial and dental sounds into one another, and the absence of all the sibilants and generally of the "noises" of speech. The phonograph is in reality a very imperfect speaker, and it requires the aid of much imagination and considerable guessing to follow its reproductions. It produces

music with wonderful perfection, but it fails to reproduce most of the "noises" of which speech is so largely made up. The telephone is also deficient in this respect, though to a much less degree.

3. The first object of the authors was to find a disk which would vibrate to the finest shades of sonorous vibrations, and which would be free from those characteristic and "personal" partials which are nearly inseparable from all vibrating disks, and which interfere with their true action. After innumerable experiments, on almost all known forms and substances, a stretched membrane of thin india-rubber rendered rigid by a cone of paper, was found to give the best effects. Such a disk was applied to the telephone and the phonograph with fair results, and the apparatus shown in fig. 1 was then constructed to record its vibrations. To the centre of the cone *ab*, shown in perspective and section in fig. 1, which was placed in a mouthpiece similar to that of a phonograph, was attached an extremely fine glass tube (*g*), which acted as a pen. The ink employed was aniline dye, and it was drawn through the pen by the very slight friction exerted between its point and the paper. The paper (*p*) on which the curves were to be drawn was the broad band frequently used for telegraphic purposes, and it was moved under the pen by mechanism similar to that used in the Wheatstone automatic telegraph apparatus, at a speed which could be varied at will from 1 to 18 inches per second.

4. In this way curves were obtained illustrating the sonorous vibrations due to the tones of speech, but their form was not so perfect as could have been wished, due to the imperfections of the disk, as well as, perhaps, to the friction of the pen failing to indicate the higher upper partials. Run at a slow speed, this instrument records the variations of air pressure in front of the lips; run at a high speed it records both air pressure and sonorous vibrations. It thus combines the functions of Barlow's logograph and Leon Scott's phonautograph.

5. It is intended, in this paper, to confine our observations to those facts illustrating vowel sounds, a graphic representation of which, drawn by the new phonautograph, is given in the following sketch (fig. 2).

6. Helmholtz's theory of vowel sounds is this:—Vowels are compound musical tones, or resultant sounds formed by the combination of certain components or simple tones called partials. The first partial, which determines the pitch of the whole, is called the prime, and the others its upper partials. The partials depend upon the reinforcements due to the cavity of the mouth. Vowels do not depend upon the pitch of the prime alone, or on the grouping or harmony of the partials alone, but on both. The ear must distinguish each component; it must recognise the kind of cavity producing the reinforcements, and therefore it determines the different vowels. This theory has been partly

confirmed recently by Messrs. Fleeming Jenkin and Ewing, by an analytical examination of phonographic tracings, fully described by them in a paper read before the Royal Society of Edinburgh.

7. The principal vowel sounds are—

Ah, as in		..	..	..	path
A	,,	..	..	..	hay
E	,,	..	..	..	he
O	,,	..	..	..	old
oo	,,	..	..	..	good.

There are several others which are modifications of these five, such as *uh* as in gut; *ă* as in bad; *aw* as in law, &c.

The diphthongs are:—

i	which is compounded	of	ah and e.
u	,,	,,	e and oo.
y	,,	,,	oo, ah, and e.

8. The cavity of the mouth changes during the articulation of these diphthongs—it remains constant during the articulation of vowels. It is thought that the influence of the first emission of breath in distinguishing the character of the vowels has been lost sight of, and that in addition to the influence of the cavity of the mouth, some allowance must be made for the increment and decrement of the sonorous vibrations, as well as for the variation of air pressure at the commencement and completion of a vowel sound. Helmholtz has acknowledged the influence of these operations in consonants and compound musical tones generally, but he has not considered them in vowel sounds. The previous diagram (fig. 2) shows what an essential feature they bear on vowel sounds.

9. The manner in which vowel sounds blend into each other is strikingly shown in the way in which different dialects deal with different vowels. Thus, what a London man calls *subject* a Lancashire man calls *soobject;* a Londoner says *Mănchester*, a Lancashire man *Mawnchester*, a Scotchman *Monchester*. *Under* is often pronounced *ŏnder*. We need not, however, examine different dialects to discover this curious blending of vowel sounds; it is found in inhabitants of the same district to a greater or less extent. Thus with the word *Manchester*, Londoners often say *Menchester*, *Manchester*, or *Marnchester*. In every case which the authors have investigated, this change of vowel sound, due to dialect, is simply due to the shifting or lowering of the upper partials.

10. The order of the principal vowels, which is given above, does not follow any theoretical principle. It would seem that a better order to follow would be one dependent on the *pitch* of the partials *as given* by Helmholtz.

1.	oo	
		} uh—(gut).
2.	o	
		} aw—(law).
3.	ah	
		} ă —(bad).
4.	ā	
5.	e	

All the subsidiary vowels, such as uh, aw, ă, take up intermediate positions in this scale, so that, in fact, we may say that there is a vowel spectrum, in which the different sounds merge into each other by almost imperceptible gradations, and hence, probably, the difference in dialectical pronunciation.

11. In the following investigation, a method opposite to that of Messrs. Fleeming Jenkin and Ewing has been adopted, *i.e.*, the question has been attacked by the method of synthesis. It has been assumed that vowels are compounded of a prime sound and certain upper partials, and the number of these partials has, for convenience, been taken as 8, although there are many more. Indeed, we have taken in some cases, the 10th, 12th, and 16th. Now, since each partial can be considered as a simple harmonic curve, if we assume the pitch of a prime to be constant, then it would be possible, by means of a machine, to represent and vary each partial in phase and in amplitude. For this purpose an instrument was constructed, which we call "the synthetic curve machine," in which a number of toothed wheels, A, B, C, D, E, F, G, H, &c. (figs. 3 and 4) are mounted on steel pins or axes rigidly fixed on a board, so that they will revolve together, and the numbers of their teeth are so calculated that during one revolution of the wheel A, B will make two, C three, D four, E five, F six, G seven, H eight revolutions, and so on. The wheel I has, on its prolonged axis, a small crank, by means of which the whole system of wheels can be rotated. On the same axis is a pinion I′, gearing into the wheel J, which, by means of a chain T, gives motion to a sliding table R. Each head of the pins on which the eight wheels revolve, has, in its centre, a small pit or hollow, in which rest the pointed ends of eight steel rods (one of which B′ only is represented in fig. 4), held in position by eight springs *b*. To the rod on the wheel A is attached, near its point, one end of a silken thread *b*′, passing over the roller N′, the other end being attached to the rod on wheel B. The rods on wheels C and D, E and F, G and H are similarly connected. The four rollers N are mounted on two levers U and U′, and these are connected by links to the lever V, which is finally linked to the lever P. This lever P is pivoted at *p*, and by means of the spiral spring S keeps the levers, links, and silk threads in a state of tension. On the longer end of the lever P is pivoted another lever O, which carries at its shorter end

a small counterbalancing weight (W), and at its longer end a glass pen (Q) containing suitable ink. On the table R is placed a piece of paper or smoked glass, which is held there by two spring clamps. Each of the eight wheels has on its face a number of small holes or pits, into which the points of the rods B′ can be placed, and these are arranged in eight rows radiating from the centre. When one of the rods, for instance that belonging to the wheel B, is placed in position B″, as indicated by the dotted lines, and motion is given to the wheels by means of the crank on the axle belonging to the wheel I, the crank-like movement of the rod B′ will, by means of the silk thread *b*′, roller N′, levers U, V, and O, cause the pen Q to move to and fro with simple harmonic motion, while the table R will move longitudinally, the pen thereby writing on the paper a simple harmonic curve. This can be done with each of the eight rods separately, the result being in each case a simple curve. Should, however, two or more rods be placed on the faces of the wheels, the result will be a curve compounded of the sum of the several simple curves. In order to increase or decrease the amplitude of a curve, the steel rods are placed further from, or nearer to, the centre of the wheels. Difference of phase is obtained by shifting the rods to the different radial rows of holes on the face of the wheels. Three additional wheels, K, L, M, have been fitted, making 10, 12 and 16 revolutions respectively, to one turn of the wheel A, and the rods belonging to neighbouring wheels are so arranged that they can be borrowed for the use of these smaller wheels if desirable.

12. Besides assuming the pitch to be constant, it has also been assumed that each octave of the partial, to maintain equal loudness of sound, must diminish one half in amplitude as it rises. Thus the

First Octave	is	$\frac{1}{2}$	the amplitude of	the prime.
Second	„	$\frac{1}{4}$	„	„
Third	„	$\frac{1}{8}$	„	„
Fourth	„	$\frac{1}{16}$	„	„

The intermediate notes, such as the third and the fifth, decrease in intermediate ratio.

13. This instrument enables us to form synthetically all the curves produced by vowel tones, and to show how these tones are compounded of primes and harmonic upper partials. It shows how *simple tones* can be produced by simple harmonic curves, and *compound tones* by the simultaneous action of several simple tones.

The following figure (fig. 5), shows the simple harmonic curve produced by each wheel, and several examples of curves formed by different components. In this way curves have been reproduced as shown in fig. 6, representing the vowel sounds based on Helmholtz's theory, as indicated by Mr. Ellis in a tabular statement, at page 181 of his translation of Helmholtz's work.

Figs. 7 and 7A show reproductions of the vowel *O*, sung at different pitches, as determined by Messrs. Fleeming Jenkin and Ewing.

14. [It is worth remarking parenthetically, that one interesting fact arising from the operation of this machine, was that curves could be so constructed as to give a stereoscopic effect. One curve was drawn simple, and the other, drawn in the same line—at the proper distance from it to fit a stereoscope—was made compound, by the addition of a partial of low amplitude. The result of the combination by the eye in a stereoscope of these two curves, was to produce a perspective effect. By this means curves have been drawn which interlace amongst each other, giving stereoscopic effects in a manner which is unique and interesting. This has no bearing whatever on the investigation, and is only adduced as a scientific toy arising out of the enquiry.]

15. Having thus studied the formation of vowel sounds, and having a means to reproduce the compound curves which graphically represent the motions which the air particles assume under their influence, the authors determined to try to reproduce these vowels by superimposing partials on to a given prime.

Since vowels are produced by a prime and its upper partials, and as the upper partials diminish so rapidly in amplitude, the idea arose that these vowels might be reproduced by sounding a prime and one of its partials alone. This was done by means of an electro-magnet E, fig. 8, vibrating an armature (A) with a moveable spring (S) attached to it in such a way that the vibrations of the armature could produce a given prime, while the vibrations of the spring, by varying its length, could also be adjusted to any particular partial.

16. The result was to roughly reproduce the principal vowel sounds, but the effect not being by any means perfect (due to the absence of the other upper partials), a machine was made on the principle of the synthetic curve machine, which would, instead of drawing curves on paper, reproduce eight partials by transferring the vibrations of the intermediate wheels to a vibrating diaphragm. This machine consists of eight wheels fixed on the same axis, the periphery of the wheels being cut into teeth of such a number as to represent the eight partials. Each tooth is a simple harmonic curve, and each wheel represents one partial. The axis can be rotated by a crank at any given velocity. By depressing a key a spring can be brought into contact with the edge of each wheel, and be thus vibrated. The vibrations of these springs are transferred by thin cords and intermediate linking to a diaphragm of ebonite. Each spring can be depressed separately or simultaneously with others, and the disk will vibrate to the resultant effect of all the vibrations. Thus, notes and chords can be sounded.

17. Here again, though the vowels were fairly reproduced, some-

thing was wanting in their clearness. This instrument proves to be an excellent syren, and all the facts illustrated by the apparatus of Cagniard de la Tour and others can be equally illustrated by it. Moreover, it forms the basis of a new musical instrument which there has been no time as yet to mature.

18. In the hope of getting more perfect definition, another machine was now made upon which disks were fitted, whose peripheries were cut in exact copy of the curve produced by the synthetic curve machine. These curves were transmitted by vibration to the receiving diaphragm of a phonograph, and really formed an "automatic phonograph." The automatic phonograph consists of an axle A, fig. 9, about 6 inches long, one end of which carries a fly-wheel B, and the other end a grooved pulley C, round which a band or gut passes from a driving wheel D, fitted with a crank handle E. On rotating the driving wheel, the long axle is caused to make about three revolutions to one of the wheel.

On the long axle are placed, in such a manner that they can easily be removed and replaced by others, a number of brass wheels or disks, *a*, *a*, *a*, *a*, the circumferences of which have been cut by a machine especially devised for that purpose into the different curves corresponding exactly to the curves obtained by the synthetic curve machine, but on a much reduced scale.

A diaphragm G with spring and frame H, similar to that in a phonograph, is so fitted that it can be shifted from one disk to another, and the sounds produced by the different curves can be readily compared. The number of periods or resultant vibrations recurring on each wheel or disk has for convenience been taken at thirty. Thus, when the driving wheel is rotated about twice per second, 180 to 200 vibrations are caused, resulting in a note at *f* or *g* in the musical scale.

A number of combinations of curves has been cut on the circumferences of the brass disks, representing each vowel sound with certain variations of the partials, as experience determined. These disks were then placed on the axle, and the sounds most resembling the vowel sounds of the human voice were easily recognised.

19. In this way it was found that from about *f* to *b* in the musical scale, the sound *oo* consists mainly of the first partial or prime. But to maintain the *oo* character descending the scale, the second and third partials became slightly necessary.

20. The prominent partial in the vowel sound *O* at the same pitch is the second, while the first can be reduced considerably. The third and fourth partials have to be used as the sound descends the scale, otherwise what is *O* at say *b* flat, will become *oo* an octave lower.

21. The vowel sound *ah* is the easiest to reproduce. It consists chiefly of the third, fourth, fifth, and sixth partials at the above pitch, the first and second partials being only slightly represented. A little

more prominence to the second, third, and fourth partials will result in *aw*, while a bright *ah* is obtained by increasing the amplitude of the fifth and sixth partials.

22. A very good and full *ah* is obtained by having all the partials equally represented, from the first to the eighth; and this really probably takes place when the human voice pronounces this vowel, as, in so doing, the mouth cavity is fully opened, so as to favour most of the partials.

23. The vowel sounds *ā* and *ee*, when reproduced by most of the ordinary phonographs, resemble respectively more *o* and *oo*. Also the curves for *a* and *ee*, obtained by the phonautograph, fig. 2, resemble those for *o* and *oo*. This shows, in the first instance, that neither instrument is sensitive to the higher upper partials; and, secondly, that the lower partials for *a* must be similar to those in *O*, and the lower partials for *ee* must be the same as in *oo*. To prove this, two disks were cut, one with a curve composed of the first, second, and eighth partials, and the other of the first, third, and eighth partials. The former, when sounded, produced a sound like *ee*, and the latter more like *a*.

24. The best *ee* has been obtained from a curve composed of the best first, second, eighth, and sixteenth partials; and *ā* from a curve composed of the first, third, and sixth or eighth partials; but this last curve can hardly be called satisfactory.

25. Diagram 10 graphically illustrates the above facts, and the following table gives them in a tabulated form:—

Vowels.	Partials with their Intensities.						
oo	1.	2.	3.					
	ff.	mf.	pp.					
ah	1.	2.	3.	4.				
	mf.	f.	mf.	p.				
o	1.	2.	3.	4.	5.	6.	8.	
	p.	p.	p.	mf.	mf.	p.	p.	
a	1.		3.				8.	
	mf.		mf.				ff.	
ee	1.	2.					8.	16.
	mf.	p.					p.	mf.

Hence, although the reproduction of vowels was good, it was imperfect. This is due probably to the absolute impossibility of reproducing the noises that accompany the last two vowels.

26. One very curious result arising from the experiments with the automatic phonograph was to show that, by varying the pitch, the vowel sounds could be shifted, *i.e.*, the curve which produced *oo* at a low velocity becomes approximately *O* at a higher velocity. *O* similarly becomes *ah*, *ah* becomes *ā*, and *ā*, *ēē*.

27. It follows from this investigation as far as it has gone, that our knowledge of vowel sounds is not perfect. The principal proof of this is the fact that vowels cannot be reproduced exactly by mechanical means. Something is always missing—probably the noises due to the rush of air through the teeth, and against the tongue and lips.

28. The curves (fig. 10) arrived at synthetically do not differ very materially from those arrived at analytically by Helmholtz (fig. 6). They principally differ in the prominence of the prime. But the prime can be dispensed with altogether. Curves produced by the synthetic machine, compounded of the different partials without their prime, show that there exist *beats* or resultant sounds. A vowel sound of the pitch of the prime may be produced by certain partials alone, without sounding the prime at all. The beat in fact becomes the prime. This point is clearly illustrated, orally, by the automatic phonograph, and graphically by the sketch (fig. 11), drawn by the synthetic curve machine. In fact, every two partials of numbers indivisible by any common multiple, if sounded alone, reproduce by their beats the prime itself. Thus, the third and the fifth partials, or the second and the third, &c., will result in the reproduction of the prime. In fact, fig. 11 illustrates not only this, but it shows that when the number of partials introduced is increased, the beats become more and more pronounced.

II.—*The Loudness of Sound.*

29. Another point remaining for investigation arising out of this inquiry, is the true theory of the loudness of sound. It is thought by the authors that loudness does not depend upon amplitude of vibration only, but also upon the quantity of air put into vibration; and, therefore, there exists an absolutely physical magnitude in acoustics analogous to that of quantity of electricity or quantity of heat, and which may be called the quantity of sound. This can be shown experimentally by constructing three disks like those in fig. 1, whose diameters increase in arithmetical ratio. When these disks are vibrated by the same curve by the automatic phonograph, or when they are thrown into vibration by tuning forks, it will be found that the intensity of sound increases in a surprising ratio. The amplitude remains just the same; the area under vibration alone increases. Thus, in the automatic phonograph, for two notes, one of which is an octave higher than the other, the area ought probably to be diminished one-half for the higher to produce equal loudness. Similarly for the same note, if we increase the area to be vibrated in its reproduction, it will be found that, as the area increases, so does the loudness of the sound emitted. In fact, in the automatic phonograph the diameter of the sounding disk ought, if it were possible, to vary with the pitch of each note, to produce equal intensity of sound.

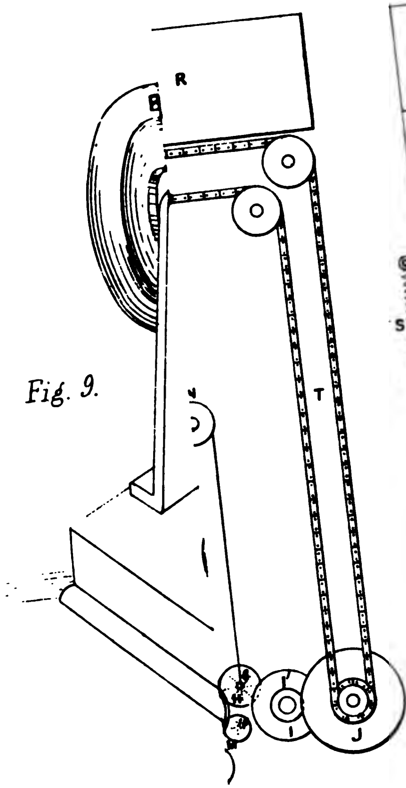


Fig. 9.

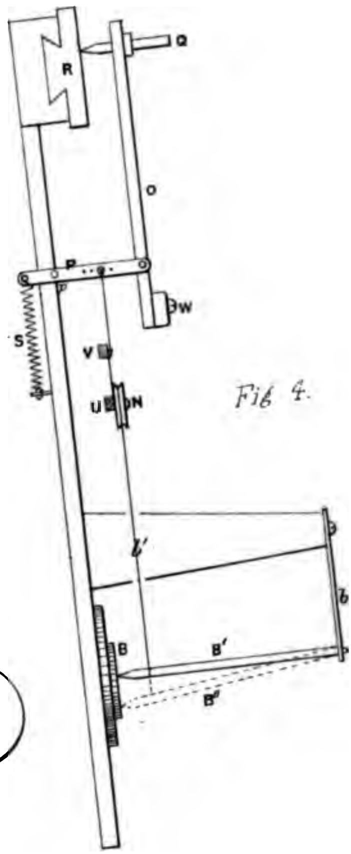


Fig 4.

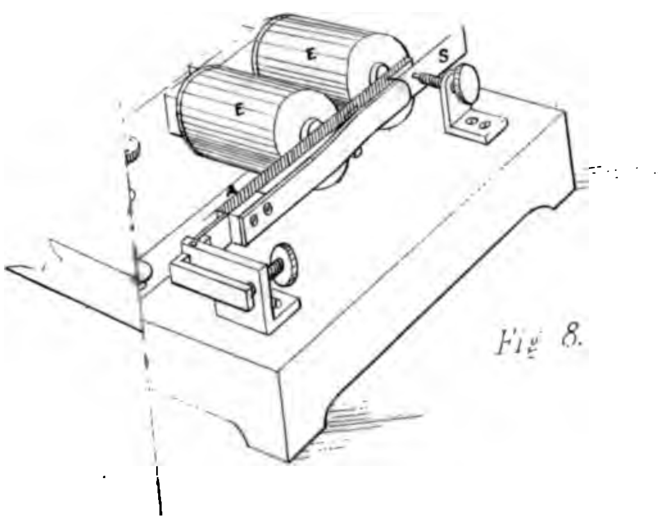


Fig 8.

[redacted]

The authors are now engaged in pursuing this inquiry into the consonantal sounds.

II. "On the Reversal of the Lines of Metallic Vapours." No. V. By G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received February 20, 1879.

Since our last communication we have continued our experiments, using the electric arc as a source of heat, in lime and in carbon crucibles as described before. Success depends on the getting a good stream of vapour in the tubular part of the crucible. This is easily attained in the lime crucibles, which quickly reach a very high temperature, but are very soon destroyed; not so certainly in the carbon crucibles, which are good conductors of heat. The latter, however, last for a very long time.

In our experiments with tubes heated in a furnace we used a small spectroscope with a single prism, which gave a good definition and plenty of light; but in the experiments here described we have used a larger spectroscope by Browning, with two prisms of 60° and one of 45°, taking readings on a graduated circle instead of on a reflected scale.

Both in the lime and in the carbon crucibles we have found that the finely channelled spectrum, extending with great uniformity from end to end, always made its appearance so long as the poles were close together. A few groups of bright lines appear on it. We have not at present investigated this remarkable spectrum further. In several cases we have observed the absorption lines of the metals put into the crucibles on this channelled spectrum as a background, but generally when the vapours in the crucibles become considerable, the channellings give place to a spectrum of bright lines on a much less bright continuous background; we have used generally thirty cells in the galvanic battery, sometimes only twenty-five, once forty.

The calcium line with wave-length 4,226 almost always appears more or less expanded with a dark line in the middle, both in the lime crucibles and in carbon crucibles into which some lime has been introduced; the remaining bright lines of calcium are also frequently seen in the like condition, but sometimes the dark line appears in the middle of K (the more refrangible of Fraunhofer's lines H), when there is none in the middle of H. On throwing some aluminium filings into the crucible, the line 4,226 appears as a broad dark band, and both H and K as well as the two aluminium lines between them appear for a second as dark bands on a continuous background. Soon they appear as bright bands with dark middles; gradually the dark line disappears from H, and afterwards from K,

while the aluminium lines remain with dark middles for a long time. When a mixture of lime and potassium carbonate (to produce a stronger current of vapour in the tube) was introduced into a carbon crucible the calcium (?) line with wave-length 4,095 was seen strongly reversed, and the group of three lines with wave-lengths 4,425, 4,434, and 4,454 were all reversed, the least refrangible being the most strongly reversed, and remaining so the longest, while the most refrangible was least strongly reversed and for the shortest time.

Besides these reversals, which were regularly observed, the following were noticed by us as occurring in lime crucibles but with less certainty, perhaps only at the highest temperatures. Dark bands appearing for a short time and dwindling into sharp dark lines with wave-lengths about 6,040 and 6,068 (perhaps due to the oxide); a dark line replacing the most refrangible of a well-marked group of several bright lines with wave-length 5,581 (or possibly the brighter line 5,588); and the lines with wave-lengths 6,121 and 6,161 reversed simultaneously for an instant and reappearing bright immediately; and the line with wave-length 5,188 reversed. When aluminium was put into the crucible only the two lines of that metal between H and K were seen reversed. The lines at the red end remained steadily bright.

When some magnesium was put into a lime crucible, the *b* group expanded a little without appearing reversed, but when some aluminium was added, the least refrangible of the three lines appeared with a dark middle, and on adding more magnesium the second line put on the same appearance; and lastly, the most refrangible was reversed in like manner. The least refrangible of the three remained reversed for some time; and the order of reversibility of the group is the inverse of that of refrangibility. Of the other magnesium lines, that in the yellowish-green (wave-length 5,527) was much expanded, the blue line (wave-length 4,703), and a line still more refrangible than the hitherto recorded lines, with wave-length 4,354, was still more expanded each time that magnesium was added. These last two lines expanded much more on their less refrangible than on their more refrangible sides, and were not seen reversed. The bright blue line (wave-length 4,481) seen when the spark is used, was not visible either bright or reversed; and this seems to be in agreement with Capron's photographs, which show this line very strong with the spark but not with the arc.

The following experiments were made in carbon crucibles:—

When strontia was put in the lines with wave-lengths 4,607, 4,215 and 4,079 were all seen with dark lines in the middle, but no reversal of any strontium line less refrangible could be seen. After adding some aluminium and some potassium carbonate to increase the current of vapour, no reversal of any strontium red line could be detected, though momentary cloudy dark bands were seen in the red when

fresh strontia was thrown in. Two dark lines were seen in the extreme red, which proved to be the potassium lines reversed (wave-lengths 7,670 and 7,700).

With a mixture of barium and potassium carbonates the line with wave-length 5,535 was strongly reversed, and that with wave-length 4,933 distinctly so. When barium chlorate was dropped into a crucible, the four lines with wave-lengths 4,553, 4,933, 5,535, and 5,518, were reversed, and as they remained so for some time, it is probable that the action of the oxygen of the chlorate had nothing to do with the result. The last-named line (5,518) was the least strongly reversed.

To observe particularly the effects of potassium a mixture of lime and potassium carbonate previously ignited was thrown in. The violet lines of potassium, wave-length 4,044, came out immediately as a broad black band, which soon resolved into *two* narrower dark bands having wave-lengths nearly 4,042 and 4,045. On turning to the red end the two extreme red lines were both seen reversed. No lines of potassium between the two extremes could be seen reversed, but the group of three yellow lines were all expanded though not nebulous, and other lines in the green were seen much expanded. These observations on potassium were more than once repeated with the same results.

Using sodium carbonate only the D lines were seen reversed though the other lines were expanded, and the pairs in the green had each become a very broad nebulous band, and D almost as broad a black band. When sodium chlorate was dropped into a crucible, the pair of lines with wave-lengths 5,681, 5,687, were both momentarily reversed, the latter much more strongly than the former.

When a very little charred rubidium tartrate was put in, the two violet lines were sharply reversed, appearing only as black lines on a continuous light background. Turning to the red end, the more refrangible of the two lines in the extreme red (wave-length 7,800) was seen to have a decided dark line in the middle, and it continued so for some time. The addition of more rubidium failed to cause any reversal of the extreme red line, or of any but the three lines already mentioned.

On putting some lithium carbonate into the crucible, the violet line of lithium appeared as a nebulous band, and on adding some aluminium this violet band became enormously expanded, but showed no reversal. The blue lithium line (wave-length 4,604) was well reversed, as was also the red line, while a fine dark line passed through the middle of the orange line. On adding now a mixture of aluminium filings, and carbonates of lithium and potassium, the red line became a broad black band, and the orange line was well reversed. The green line was exceedingly bright, but not nebulous or reversed, and the violet line still remained much expanded, but unreversed. With regard to the green lithium line, we may remark that we have no doubt whatever that it belongs to lithium, and that there must have been some

mistake in Thalén's observation, which ascribed it to cæsium. We have never detected this line with cæsium, which, on the other hand, seems always to give the characteristic blue lines, both in the spark and in the flame, as well as to give the same lines reversed when its vapour is used as an absorbent.

When metallic indium was introduced into the crucible, both the lines with wave-lengths 4,101 and 4,509 were at once seen strongly reversed, and so continued for some time. No other absorption line of indium could be detected.

It is apparent that the expansion of lines, so often observed when fresh materials are introduced, must be ascribed to increase in the density of the vapours, not to any increase of temperature. Moreover, the length of tube which reaches a very high temperature in the experiments above described is very short in the lime crucibles, and still shorter in the carbon crucibles, so that the reversing layer is also short in many cases. We are, therefore, directing our attention to the means of heating up a longer length of the tubes, either by introducing oxy-hydrogen jets, or additional electric arcs one above another; and also to the introduction of reducing gas (hydrogen or carbonic oxide) to counteract the oxidising action of the air which is drawn in through the lateral openings.

The curious behaviour of the lines of different spectra with regard to reversal has induced us to compare the bright lines of the chromosphere of the sun, as observed by Young, with those that are reversed in our crucibles. It is well known that some of the principal lines of metals giving comparatively simple spectra, such as lithium, aluminium, strontium, and potassium, are not represented amongst the dark lines of Fraunhofer, while other lines of those metals are seen: and an examination of the bright chromospheric lines shows that special rays highly characteristic of bodies which appear from other rays to be present in the chromosphere are absent, or are less frequent in their occurrence than others.

In the following tables the relation between our observations on reversals and Young's on the chromospheric lines is shown.

Lines in wave-lengths.		Frequency in chromosphere.	Behaviour. Reversal in our tubes.	Remarks.
Sodium..	6,160 6,154	0	Expanded.	
	D	50	Most easy	Principal ray.
	5,687 5,681	2	Difficultly reversed.	
	5,155 5,152	2	Very diffused.	
	4,983 4,982	0	" "	

Lines in wave-lengths.		Frequency in chromosphere.	Behaviour. Reversal in our tubes.	Remarks.
Lithium .	6,705	0	Reversed	Most characteristic, at low temperature and low density.
	6,101	3	Difficultly reversed.	
	4,972	0	0	
	4,603	0	Readily reversed.	
	4,130	0	Very diffused.......	Described by Boisbaudran.
Magnesium	5,527	40	Expanded.	
b_1	5,183	50	Reversed...........	Most characteristic.
b_2	5,172	50	"	
b_4	5,167	30	Difficultly reversed ..	
	4,703	0	Much expanded.	
?	4,586	0	" "	Doubtful whether due to magnesium.
	4,481	0	Not seen either bright or reversed.	Characteristic of spark absent in arc.
Barium ...	6,677	25	0	May be either Ba or Sr.
	6,496	18	0	May be either Ba or Sr.
	6,140	25	0	
	5,534	50	Readily reversed	Most persistent.
	5,518	15	Reversed.	
	4,933	30	"	Well-marked ray.
	4,899	30	0	
	4,553	10	Pretty readily reversed.	
Strontium.	6,677	25	0	May be Sr or Ba.
	6,496	18	0	" " "
	4,607	0	Readily and strongly reversed.	Most characteristic.
	4,215	40	Readily reversed	Well marked.
	4,077	25	" "	" "
Calcium...	6,161	8	Reversed difficultly ..	Very bright.
	6,121	5	" "	
	5,587	2	Doubtful reversal.	
	5,188	10	Reversed.	
	4,587	2	0	
	4,576	4	0	
	4,453	0	Readily reversed.	
	4,435	1	" "	
	4,425	2	" "	
	4,226	3	Most easily reversed .	Very characteristic.
	4,095(?)	0	Strongly reversed.	
	3,968	75	Well reversed.	
	3,933	50	Rather more readily than the last.	
Aluminium.	6,245	8	0	Strong lines.
	6,237	8	0	
	3,961 3,943	0	Strongly reversed....	Very marked.

Lines in wave-lengths.		Frequency in chromosphere.	Behaviour. Reversal in our tubes.	Remarks.
Potassium..	7,670 } 7,700 }	0	Strongly reversed....	Chief rays.
	4,044 } 4,042 }	3	....	Well marked.
Cæsium....	5,990	10	0	
	4,555	10	Strongly reversed....	Most marked.

In a subsequent communication we intend to examine carefully the contents of the preceding table. In the meantime we may remark that the group calcium, barium, and strontium, on the one hand, and sodium, lithium, magnesium, and hydrogen, on the other, seem to behave in a similar way in the chromosphere of the sun.

Presents, February 6, 1879.

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March 6, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates for election into the Society were read, as follows:—

Henry James Alderson, Lieut.-Col. R.A.

Thomas Clifford Allbutt, M.A., M.D.

John Anderson, M.D., F.R.S.E., F.L.S.

William Edward Ayrton, Assoc. Inst. C.E.

Henry Walter Bates, F.L.S., F.Z.S.

Rev. Miles Joseph Berkeley, F.L.S.

Henry Bessemer, Assoc. Inst. C.E.
Henry Francis Blanford, F.G.S.
George Stewardson Brady, M.D., F.L.S.
Prof. Alexander Crum Brown, D.Sc., M.D.
Walter Lawry Buller, D.Sc., F.L.S.
Charles Creighton, M.D.
William Sweetland Dallas, F.L.S.
George Howard Darwin, M.A.
Francis Stephen Bennet François de Chaumont, M.D.
John Dixon, C.E.
Sir George Duckett, Bart.
Prof. Joseph D. Everett, M.A., D.C.L.
William Galloway.
Henry Haversham Godwin-Austen, Lieut.-Col.
Prof. Thomas Minchin Goodeve, M.A.
Charles Alexander Gordon, M.D., C.B.
Charles Graves, Bishop of Limerick.
Townshend Monckton Hall, F.G.S.
John Harley, M.D., F.L.S.
John Deakin Heaton, M.D.
Henry M. Jeffery, M.A.
John Edward Lee, F.S.A., F.G.S.
Prof. George Downing Liveing, M.A.
George Matthey, F.C.S.
William Munro, General, C.B., F.L.S.
Charles Henry Owen, Col. R.A.
William Henry Preece, C.E.
Charles Bland Radcliffe, M.D., F.R.C.P.
John Rae, LL.D.
George Banks Rennie, C.E.
Prof. J. Emerson Reynolds, M.D.
George F. Rodwell, F.C.S.
George John Romanes, M.A.
Sir Sidney Smith Saunders, C.M.G.
Arthur Schuster, Ph.D., F.R.A.S.
Michael Scott, M.I.C.E.
Prof. Harry Govier Seeley, F.L.S.
John Spiller, F.C.S.
Bindon Blood Stoney, M.A., M.I.C.E.
Sir Henry Thompson, F.R.C.S.
William A. Tilden, D.Sc.
Alfred Tribe, F.C.S.
James Clifton Ward, F.G.S.
Benjamin Williamson, M.A.
Charles R. Alder Wright, D.Sc.
Prof. Edward Percival Wright, M.D., M.A., F.L.S.
Thomas Wright, M.D., F.R.S.E., F.G.S.

The following Papers were read:—

I. "Observations on the Physiology of the Nervous System of the Crayfish (*Astacus Fluviatilis*)." By JAMES WARD, M.A., Fellow of Trinity College, Cambridge. Communicated by MICHAEL FOSTER, M.D., F.R.S., Prælector of Trinity College, Cambridge. Received February 17, 1879.

I. *When one of the supra-œsophageal commissures is divided*, the whole body of the crayfish on the injured side is more or less enfeebled, with

the exception of the swimmerets and possibly the gnathites. The change is most marked in the antennæ and eye-stalks, which barely respond to considerable excitation; and after these perhaps in the abdomen, the power of swimming or turning over being generally entirely lost. The muscles connecting the abdominal segments on the injured side are relaxed, and the tail-fin appendages on that side are no longer spread out in the normal manner, but remain more or less overlapping and hang down like broken limbs. This leads to a want of symmetry which is most conspicuous during movement: it almost disappears when the nervous connexion with the abdomen is entirely severed by a cut between the first and second segments. No clear difference is discernible in the pinch of the two chelæ, but in prehension and locomotion all the limbs on the side of the injury are weakened. In consequence of this, when walking forward the course taken is towards the sound side, in backing the course is towards the injured side. The chelæ during progression show a bias towards the sound side; that is to say, when the right commissure is cut, they are both directed towards some position on the animal's left, and *vice versâ* when the left commissure is cut. There is a tendency when walking to flop suddenly forwards, and in some cases to "wabble" from side to side.

II. So long, however, as the other commissure remains intact, there is no lack of spontaneity and purpose in the movements of the cray-fish; but when this too is severed, *that is, when both commissures connecting the supra- with the sub-œsophageal ganglion are divided*, everything of the kind disappears, save that occasionally the antennæ are waved about in the normal fashion, though much more feebly. The animal lies on its back, the maxillipedes, the chelæ, and the first three pair of legs, for the most part, swinging slowly to and fro in perfect *tempo;* not, however, as the swimmerets do, both sides synchronously, but with the movements of one side alternating with those of the other. On a very slight disturbance, and at intervals, without any obvious cause, this rhythmic swing gives place to feeding or "preening" movements, the last being chiefly confined to the fourth pair of legs, which take no part in the rhythmic swing. The feeding movements are a perfect mimicry of the movements made when food is actually seized. These last appear to be in all respects perfectly co-ordinated; so much so, indeed, that the chelate legs will wait their turn to pass their morsel to the mouth when scraps are placed in all of them at once. But neither they, nor the chelæ, nor the posterior maxillipedes, show any selective power, even the animal's own antennæ being seized: the first evidence of taste appears when the food gets within the gape of the mandibles.

When placed on a table, the ambulatory legs are straightened out so as to lift the body as if upon stilts, the half flexed abdomen barely

touching the ground with the tail-fin. In this position the animal will remain for a minute or so, one or more of the chelate legs engaging in feeding movements, while the last pair are doing their best to preen the abdomen. At length there is an attempt at locomotion, the limbs being moved slowly and in a tottering fashion, though with fair co-ordination, till after a few steps, having no power to recover its equilibrium, the animal rolls over helplessly on to its back. In some cases the chelæ were folded rigidly across each other so as to render locomotion impossible.

III. *When both commissures are divided behind the sub-œsophageal ganglion*, the antennæ are moved more frequently and more vigorously than in the last case: the eye-stalks too are oftener in motion. The rhythmic swing is not infrequent in the posterior maxillipedes, but very exceptional and of very short duration elsewhere Preening movements are more common than under the last head, and in these all four pairs oftener take part; but feeding movements, save after external excitation, are quite exceptional. Then, however, they are vigorous enough, but the chelate legs are very uncertain in their aims at the mouth, do not loose their hold of the food when they get it there, and all of them attempt to crowd food into the mouth together. But the food is frequently rejected: in two cases out of three in which the experiment was tried, this "sulkiness" disappeared on dividing the supra-œsophageal commissures.

On the table these crayfish are unable to support themselves, the chelæ sprawl helplessly on either side and the legs are for the most part doubled up under the body. The posterior maxillipedes alone retain their wonted strength, and by means of these the cephalothorax is raised from the ground two or three times a minute till they are exhausted; the antennæ too being waved vigorously all the time.

IV. In three cases in which *a longitudinal division of the supra-œsophageal ganglion* was accomplished fairly satisfactorily, the animal assumed the stilted position above described, but the abdomen, instead of being bent sharply downwards, was alternately elevated to the utmost and then depressed and sometimes curved rigidly backwards for a minute or more: at which times, owing to the *rigor* of the chelæ, it was possible to make the animal stand upon its head. These animals had considerable power of maintaining equilibrium and were active in the water, making, however, very pronounced "circus-movements." Their ambulatory legs were always obedient to the impulse to walk, and never betook themselves to feeding or preening movements at such times.

From the foregoing it may perhaps, with more or less probability, be inferred:—

(*a.*) That there is no decussation of the longitudinal fibres in the nervous system of the crayfish.

(*b.*) That on the presence of the supra-œsophageal ganglion depend (1) the spontaneous activity of the animal as a whole, or what might be called its volitional activity; (2) the power to inhibit the aimless and wasteful mechanical activity of the lower centres; (3) the power to maintain equilibrium; and (4) the use of the abdomen in swimming.

(*c.*) That the sub-œsophageal ganglia are the centres for co-ordinating (1) the locomotive* and (2) the feeding movements, and (3) for the rhythmic swing described under II. (The stilted gait in II and the vigour of the posterior maxillipedes in III, the limbs connected with the other centres being then disabled for locomotion, seem to show that the sub-œsophageal ganglion is the source of a considerable amount of motor energy.)

(*d.*) That there is much less solidarity, a much less perfect *consensus*, among the nervous centres in the crayfish than in animals higher in the scale. The brainless frog, *e.g.*, is motionless except when stimulated, and even then does nothing to suggest that its members have a life on their own account; whereas the limbs of a crayfish deprived of its first two ganglia, are almost incessantly preening, and when feeding movements are started, the chelate legs rob, and play at cross purposes with, each other as well as four distinct individuals could do.

(*e.*) That some stimulus from other centres is more or less necessary to the activity of any given centre. This conclusion is rendered, at all events, probable (1) by a comparison of the activity of the antennæ and eye-stalks in I, II, and III; (2) by the diminution in the spontaneous feeding movements in III; and (3) by the simultaneous increase in the preening movements—the excitations from the tail-fin region having no longer a counterpoise.

(*f.*) The "natural" discharge of a ganglionic centre (not exhibiting "volition") appears to be of a rhythmic kind; the rhythmic movements becoming converted into varied movements by temporary augmentation or inhibition.†

It remains to mention one or two outlying points. There is much in the action and inaction of the mandibles, to suggest very considerable independence between the centre for their movements and that for the movements of the maxillipedes—which last is doubtless situated in the sub-œsophageal ganglion. Thus the mandibles in several cases lost the power to move while the maxillipedes continued unaffected, and

* In further proof of this position it may be added that, when the commissures are divided behind the second thoracic ganglia, the animal crawls with extreme difficulty by alternate advances of the chelæ alone; and that when they are divided behind the third it walks by alternate advances both of the chelæ and the first pair of legs: the other legs in each case being rucked together in confusion.

† Is there such a rhythm at the bottom of "volitional" movements?

they never, at any time, participated in the rhythmic swing or feeding movements of these last.

Gentle pressure on the anus or the sexual organs *excites or inhibits* the swimmerets, according as they are already at rest or in motion, and leads, where possible, to a folding of the abdomen. The feeding and preening movements are also, as a rule, brought to a complete standstill by slight irritation of the anus, the after movements being in all cases more violent. So long as the nervous connexion with the tail-fin remained intact, the swimmerets can be excited to considerable activity by touching this region, but when this connexion is destroyed, it is with difficulty they are made to move at all.

The experiments, of which the above is a brief and preliminary account, were carried on at the Physiological Laboratory, Cambridge.

II. "Preliminary Report upon the *Comatulæ* of the 'Challenger' Expedition." By P. HERBERT CARPENTER, M.A., Assistant Master at Eton College. Communicated by Sir WYVILLE THOMSON, F.R.S. Received February 18, 1879. Published by permission of the Lords Commissioners of the Treasury.

The collection of *Comatulæ* made by the staff of the "Challenger" includes specimens from 45 different localities, but few of which are deep-water stations. *Comatulæ* were only obtained seven times from depths exceeding 1,000 fathoms, namely at:—

Station.	Depth.	Station.	Depth.
205	1,050 fathoms	158	1,800 fathoms.
218	1,070 ,,	160	2,600 ,,
175	1,350 ,,	244	2,900 ,,
147	1,600 ,,		

At lesser depths, 200—1,000 fathoms, *Comatulæ* were met with at 13 stations; but by far the greatest number both of species and of individuals were dredged at depths much less than 200 fathoms, and often less than 20 fathoms, at 26 widely distant stations.

No.	Station.	Locality.	Depth in fathoms.	Antedon.	Actinometra.	Promachocrinus.	Ophiocrinus.
1	48	....	51	2			
2	..	St. Paul's Rocks...........	10—80	..	1		
3	122	....	350	1			
4	..	Bahia.....................	7—20	1	2		

No.	Station.	Locality.	Depth in fathoms.	Antedon.	Actinometra.	Promachocrinus.	Ophiocrinus.
5	135	Off Tristan d'Acunha	550	1			
6	..	Simon's Bay.............	10—20	..	1		
7	..	Marion Island............	50—75	1			
8	147	....	1,600	2	..	1	
9	149	Kerguelen { Balfour Bay ... Royal Sound... Cape Maclear..	20—60 28 30	..	..	1	
10	150	....	150	1			
11	151	Heard Island	75	1	..	1	
12	158	....	1,800	..	..	1	
13	160	....	2,600	1			
14	163A	Port Jackson	2—10	1	1		
15	164	....	950	1	..	..	1
16	169	....	700	..	..	..	1
17	170	....	630	4			
18	..	Tongatabu Reefs..........	..	1	1		
19	174	....	210, 255, 610	4	3		
20	175	....	1,350	3			
21	186	Torres Straits { Sept. 7, 1874 " 8, "	3—11 8	.. 2	3 6		
22	187	Cape York, Sept. 9, 1874. ..	6	1	2		
23	190	....	49	2			
24	192	....	129	11			
25	..	Aru Islands	..	1	1		
26	..	Banda....................	17	..	11		
27	..	Ternate..................	..	..	1		
28	..	Zamboanga................	10	..	14		
29	201	....	82—102	2			
30	..	Zebu Reefs...............	..	2			
31	205	....	1,050	..	..	..	1
32	208	....	18	3	1		
33	210	....	375	2			
34	212	....	10—20	5			
35	214	....	500	3	..	1	
36	218	....	1,070	1			
37	219	....	150	1			
38	..	Admiralty Islands.........	..	..	2		
39	232	....	345	1			
40	235	....	565	..	..	..	1
41	236	....	775	1			
42	244	....	2,900	1			
43	308	....	175	1			
44	320	....	600	1			
45	344	....	420	1			

At the present time I regard the collection as containing 111 species, mostly new; but as the work of examination and description progresses, it is not unlikely that forms which I now consider different may turn out to be merely local varieties of one and the same species, so that the number given above may be subject to alteration.

Of these 111 species, 59 belong to the genus *Antedon*, 48 to *Actinometra*, 1 to *Ophiocrinus*, and 3, which are peculiar in having ten rays to the calyx instead of only five, to a new genus for which I propose the name *Promachocrinus* (πρόμαχος, "Challenger.") It may be thought that this peculiarity is hardly a sufficient reason for the erection of a new genus to receive these three species. It is, however, a much more striking one than that on which the genus *Ophiocrinus* is based, viz., the presence of five arms only, as the rays, unlike those of most *Comatulæ*, do not divide but bear the arms directly. In *Promachocrinus* on the other hand, there are ten distinct rays, the radial pentagon which is in contact with the centrodorsal consisting of ten separate pieces, and not of five only, as in *Ophiocrinus* and in the other *Comatulæ*.

In two of the species the rays are undivided as in *Ophiocrinus;* but in the third they divide, as in our common *Antedon rosacea*, so that there are twenty arms.

This character, the presence of ten rays, is evidently not an accidental one, like the existence of more or less than five rays in other *Comatulæ* and in *Rhizocrinus*. In the latter genus individuals with four to six rays are common, and cases of seven, though rare, may occur. Among the *Comatulæ*, however, it is very different. I have carefully examined three large *Comatula* collections besides that of the "Challenger," viz., those of the British and Paris Museums, and Professor Semper's collection from the Philippine Islands. Out of the nearly 200 species contained in these collections I have found but two specimens in which there are not five rays in the calyx. In one of these there are only four, and in the other six rays, though in other individuals of each species there are five, the normal number.

The distribution of *Promachocrinus* is as follows:—

P. Kerguelensis (20 arms).	Balfour Bay, Kerguelen,	20—60 fathoms.
	Royal Sound ,,	28 fathoms.
	Cape Maclear ,,	30 ,,
	Heard Island	75 ,,
P. abyssorum (10 arms).	Station 147..........	1,600 ,,
	,, 158..........	1,800 ,,
P. Naresii (10 arms).	,, 214..........	500 ,,

Ophiocrinus was obtained at four localities at depths varying from 565 to 1,070 fathoms, two in the South Pacific, off South Australia and New Zealand respectively, and two in the North Pacific, one off Japan, and one just north of the Philippine Islands. All the specimens belong to one species, which is by no means so slender and graceful as Semper's Philippine species from shallower water, but has a much more massive arm skeleton.

Among the numerous species of *Antedon* (59) and *Actinometra* (48)

the only species which I have been able to identify with any certainty are:—

Antedon Eschrichtii.	*Actinometra multiradiata.*
„ *macrocnema.*	„ *fimbriata.*
„ *Brasiliensis* (*Lütk.*).	„ *Novæ Guineæ.*
	„ *trichoptera.*

Müller's specific diagnoses are, as is well known, very incomplete; and it is possible that a personal examination of his original specimens will enable me to identify more of his species than I can at present. I am inclined to think that besides the above-mentioned species, the "Challenger" collection also includes the following:—

Act. purpurea.	*Act. Wahlberghii.*
Act. rotalaria.	*Act. stellata* (*Lütk.*).

The comparative distribution of these two genera is very striking. Relatively speaking, *Actinometra* is extremely limited in its range, both geographical and bathymetrical. It is almost exclusively a tropical genus, its northern limit being about 30° N. lat. and its southern 40° S. lat. Isolated species are known from the Cape of Good Hope, Natal, South Australia, and Port Jackson, but its chief home is Oceania, especially the Philippines and Moluccas, from which latter locality the "Challenger" brought home 11 species of *Actinometra*, but not a single *Antedon*. 14 species were found at Zamboanga, in the Philippines, but no *Antedon;* while at the Zebu Reefs, in another part of this group, two *Antedons* were obtained, but no *Actinometra;* and at Station 192, 11 *Antedons*, but no *Actinometra*, just the reverse of what was found at Banda, in the Moluccas. A few *Actinometra* species are also known from the west coasts of the Atlantic, as South Carolina, the West Indies, Bahia, and St. Paul's Rocks.

The bathymetrical range of *Actinometra* is likewise very narrow. Nearly all the "Challenger" species are from depths less than 20 fathoms, while only three come from a greater depth than 100 fathoms. These were all obtained at Station 174, where the depths of different hauls were 210, 255, and 610 fathoms. I have no information as to which of these hauls yielded the three species in question. The individual species of *Actinometra*, like the genus itself, are very local in their distribution. *Act. solaris* seems to have a fairly wide range in the Malay Archipelago and in Oceania, though oddly enough it does not occur in the "Challenger" collection. Each of the forty-eight species of this collection has its own locality. In no case have I been able to refer specimens from different localities to the same species, except that duplicates of the same species were found at two stations in Torres Straits (186, 187), very close to each other.

With *Antedon*, however, the case is different. Not only do nearly all the deep-sea *Comatulæ* belong to this genus, but some species of it

have a fairly wide range. *Ant. rosacea* ranges from the north of Scotland to the Mediterranean, while *Ant. Eschrichtii* is found over a much wider area. It is the common Arctic species, having been obtained by our own expedition under Nares, as far north as lat. 81° N., while the expeditions of Sweden, Norway, and other countries have found it abundant in the seas of Spitzbergen and Nova Zembla. It is well known on the American coast, and was dredged by the "Challenger" off Halifax, while the "Porcupine" met with it in the "cold area" of the North Atlantic.

The "Challenger" dredgings round Heard Island yielded several specimens which agree so very closely with *Ant. Eschrichtii* that I am very strongly inclined to believe in the identity of the southern and northern forms. There are, however, some minor points of difference between them, and the southern form may really turn out to be the representative species of *Ant. Eschrichtii*, but not identical with it. I cannot venture to give a definite opinion upon this point until I have had an opportunity of examining a greater variety of specimens than are accessible to me just at present.

There are other *Antedon* species, which occur in duplicate from different localities. Two specimens from near the Kermadec Islands (S. 170), also occur in the neighbourhood of the Fijis (S. 174, 175). A third species was dredged at Stations 147 and 160, two localities in the Southern Sea, in nearly the same latitude, but separated by almost 90° of longitude. A fourth species came up from 1,070 and 775 fathoms, off the Admiralty Islands and Japan respectively.

The above facts would seem to show that, with few exceptions, the geographical range of the individual members of the family *Comatulidæ*, is exceedingly limited, nearly every species having its own locality, and that not a very extensive one.

This is not surprising when it is remembered how rarely *Comatulæ* have been found at great depths. The stalked Crinoids, on the other hand, are especially characteristic of the abyssal fauna, *Pentacrinus*, *Bathycrinus*, and *Rhizocrinus*, all having a very wide distribution. This is true, also, even with the individual species of the latter genus. This accords well with our palæontological knowledge. Chalk *Comatulæ* are exceedingly rare. Hagenow found one in Germany, which he named *Hertha mystica*. From the figure which he gives of its calyx, I should judge it to be an *Antedon*, which agrees well with the facts stated above. Lundgren* has found a calyx in the chalk of Sweden, which "comes very near to *Antedon Fischeri*, Geinitz." There are also a few chalk *Comatulæ* in the Woodwardian and British Museums, viz., *Glenotremites* and similar forms, but they are as nothing compared to the remains of *Pentacrinus* and *Bourguetticrinus*, and even

* "Neues Jahrbuch für Mineralogie." Heft ii, 1876, pp. 180–182.

these are not too common. A few specimens are known from the Gault, Greensand, and Bath Oolite, while the Jurassic beds of the Continent have yielded *Solanocrinus* and a few little known forms from the Solenhofen slate (*Pterocoma*, *Saccocoma*).

It should be noted, however, at the same time, that Tertiary *Comatulæ* are also very rare, Philippi's *Alecto alticeps*, from the Sicilian Tertiaries being the only one which I can call to mind. This is scarcely surprising when it is remembered that the distribution of modern *Comatulæ* is chiefly in the tropics and temperate zones, there being but few Arctic or sub-Arctic species. The Australian Tertiaries might possibly yield different results.

The voyage of the "Challenger" has settled two curious questions in connexion with the Crinoids, the origin of which is due to Lovèn. They refer to *Hyponome Sarsii*, a so-called recent Cystid, and to *Phanogenia*, a supposed new genus of the *Comatulidæ*. *Hyponome* turns out to be nothing more than the disk of a *Comatula*, minus its skeleton. The anambulacral plating may be very extensive, forming a complete pavement over the ventral surface of the disk as in many *Pentacrini*; and the ambulacra are not wide and open as is usual in most *Comatulæ*, but almost entirely closed by the approximation of the marginal leaflets at their sides, so that the food-grooves radiating from the mouth are converted into tunnels. In Lovèn's specimen the mouth was central but almost concealed, and several similar ones were obtained by the "Challenger" at Cape York, together with one still retained in its calyx and similar in every respect to an ordinary *Antedon*. This last shows that it is only on the disk that the ambulacra are partially closed, for they are quite open and of the usual character on the arms.

Species of *Actinometra* may also exhibit this condition of more or less completely closed ambulacra on the disk. One of the most abundant *Comatulæ* at Cape York is a large *Actinometra*, the disk of which corresponds exactly to Lovèn's description of *Hyponome*, except in the eccentric position of the mouth. Since learning the true nature of *Hyponome* from Sir Wyville Thomson, I have looked out for a similar condition in other *Comatulæ*, and have found that it is not uncommon though rarely so marked as in the Cape York species.

Two species of *Antedon*, dredged by the "Challenger" at Station 214, have disks, which, if separated from their dorsal skeleton, would be very perfect *Hyponomes*. In each species the whole of the ventral perisome is covered with an extensive *anambulacral* plating, and the marginal leaflets at the edges of the grooves of both disk and arms also contain distinct plates. In most *Comatulæ* there are no plates in the marginal leaflets, or at most, a few calcareous spicules, irregularly disposed. In these two species, however, there are definite plates which are comparatively small as in *Pentacrinus*, and do not attain to

anything like the relative size of the reniform plates at the sides of the grooves of *Rhizocrinus*, *Hyocrinus*, and *Bathycrinus*. They are all folded down more or less completely over the grooves, which are thus converted into tunnels; while the mouth is also rendered more or less invisible by the folding over it of the plated leaflets around the edges of the peristome. The closure of the grooves is much more perfect in some specimens than in others and may extend far out on to the arms.

The plates in the marginal leaflets are probably moveable as the unplated leaflets are in *Antedon rosacea;* so that they can be erected when the arms are spread out, leaving the grooves open for food particles to travel towards the mouth. On the other hand, when the arms are all contracted over the disk, the marginal plates fold over the grooves and cover them in. This is the condition of most spirit-specimens, but it is not in any way comparable to that of the Palæozoic Crinoids, in which the mouth is truly subtegminal, while the ambulacra become real tunnels beneath the upper surface of the vault.

Sections through one of these plated *Hyponome*-disks show that all the various structures which underlie the grooves of ordinary *Comatulæ* are present and exhibit their usual characters.

A new *Comatula* has been described by Lovèn* under the name *Phanogenia*, which presents a very remarkable condition of the centro-dorsal piece.

Lovèn's specific diagnosis of *Phanogenia typica* commences as follows: —"*Calyx fere planus, facie dorsali totus cum brachiis lævis, suturis linearibus, facie ventrali usque ad finem brachialis secundi sulcis aratus, quibus adhaeret perisoma. Articulus centrodorsalis, verticillaris, persistens, simplex, formam servans stellæ quinquangularis minutæ, sinubus rotundatis, radiis obtusis, facie dorsali leviter convexa lævis, cirris præditus perpaucis (circ. octo. ?), in sinubus sparsis, pusillis, quintem partem diametri stellæ longitudine vix superantibus, crassiusculis, versus basin validiusculis, teretibus, leviter arcuatis, lævibus, apice muticis, caducis foveolas relinquentibus minutas medio perforatas.*" The figure accompanying the above description shows the centrodorsal in the form of a five-rayed star, which does not, however, spread out over the radials so as to conceal them more or less completely, as is usual in most *Comatulæ*, except that the points of the star just overlie the inner ends of the lines of synostosis of every two adjacent radials. The dorsal surface of the star is level with that of the rest of the calyx, and is marked by a few cirrhus sockets, in two or three of which there are one or two very minute cirrhus stumps.

This is a very remarkable condition of the centrodorsal. In nearly

* "Phanogenia, ett hittills okändt slägte af fria Crinoideer." "Ofversigt af Kongl. Vetenskaps-Akademiens Forhandlingar." 1866. No. 9.

all the other *Comatulæ* it is circular or pentagonal, more or less convex, and marked with several cirrhus sockets, either only at the margin or all over its surface, while the cirrhi are rarely so imperfect as in Lovèn's specimen.

Among the *Comatulæ* in the British Museum is a specimen labelled *Actinometra stellata*, Ltk. It had been purchased from the collection of the Godeffroy Museum, having been previously examined and named by Dr. Lütken.

The mouth is not central as Lovèn describes it in *Phanogenia*, but eccentric, though comparatively only but slightly so. The condition of the centrodorsal, however, is essentially similar to that presented by *Phanogenia*, and it was this feature, I do not doubt, that caused Lütken to give the specific name *stellata* to this type. The stellate condition of the centrodorsal in *Phanogenia* has long been a puzzle to me, and I am therefore glad to be able to say that the material brought home by the "Challenger" throws a considerable light upon it. This condition appears to be one of the concluding stages of a long series of changes in the shape and relations of the centrodorsal, which do not commence until some time after the loss of the stem, and the entry upon the free state of existence.

The "Challenger" dredgings in Torres Straits brought up a considerable number of specimens of a hitherto undescribed *Comatula*. This species was first discovered by the late Professor Jukes, who brought home specimens and deposited them in the British Museum. I propose, therefore, to name it *Actinometra Jukesii*. The "Challenger" collection contains nine young specimens of this species, most of which have cirrhi on the centrodorsal. But in the adult the centrodorsal is a pentagonal plate four millims. in diameter, without a trace of cirrhi or even of cirrhus sockets. Its surface is level with that of the radial pentagon within which it is enclosed.

Stage 1. In the youngest specimen the centrodorsal is a nearly circular plate 1·5 millims. in diameter, just sufficiently raised above the surface of the radial pentagon to bear about eight marginal cirrhi.

Stage 2. In others from the same locality which have a centrodorsal 2 millims. in diameter, it bears no cirrhi, and the sockets are partly obliterated, while the height of the plate above the rest of the calyx is somewhat reduced. Three other specimens, however, of the same size which were obtained at Cape York on another day, still retain their cirrhi.

Stage 3. By the time that the diameter of the centrodorsal increases from 2 to 2·5 millims., its shape becomes more distinctly pentagonal and scarcely any trace of cirrhus sockets is visible, the plate being so thin that it rises very little indeed above the level of the radials. In one specimen of this size there is one rudimentary cirrhus stump and two

or three faint indications of sockets, very much as in the stellate centrodorsal of *Phanogenia.*

Stage 4. The adult condition succeeds to this. The centrodorsal is a simple pentagonal plate 3·5—4 millims. in diameter, and situated entirely within, and on the same level as, the radial pentagon.

This series of changes in the centrodorsal does not proceed any farther in *Act. Jukesii*, but in other species it may continue still farther, or on the other hand close sooner. Thus the centrodorsal of a gigantic *Actinometra* in Professor Semper's Philippine collection is in stage 2; while those of two other "Challenger" species, from the same locality and equally large, exhibit the next stage of metamorphosis (5).

Stage 5. In No. 37 (of my list) the centrodorsal is a pentagonal disk without a trace of cirrhus sockets. It is slightly *below* the level of the radials, and is only in contact with them by its inter-radial angles, its sides being separated from their inner margins by linear clefts.

No. 49 is in the same condition, and so are two others of Semper's Philippine species, except that one or two minute cirrhus stumps still remain on the centrodorsal. Another large "Challenger" species, also from the Philippines, is represented by four specimens. All of these show the gradual obliteration of the cirrhus sockets and the lowering of the centrodorsal to the level of the radial pentagon, or even below it, together with the presence of clefts at its sides. These may occasionally appear before the loss of the small cirrhus stumps, as is the case in *Phanogenia.*

Stage 6. In No. 6, another large Philippine species, the clefts are somewhat wider but very shallow. They are deeper in No. 30, but there is no trace of cirrhi on the stellate centrodorsal. Except in this point (a very variable one, as seen above) this seems to be about the condition of *Phanogenia.*

Stage 7. The last stage is reached in another of Semper's specimens which was purchased from the Godeffroy Museum, and appears to me to agree very closely with Lütken's *Act. stellata.* The centrodorsal is star-shaped, having a flat centre and five rays, the length of which is about one-third the diameter of the centre. The points of the rays abut on the radial pentagon at the synostoses of every two contiguous radials and are therefore *inter-radial.* The re-entering angles of the star are occupied by five clefts, each of which is somewhat planoconvex in shape. It is bounded centrally by the centrodorsal plate, laterally by two of its rays, and peripherally by the inner margin of a radial. These openings are large enough to admit the point of a good sized needle for a short distance.

The causes which lead to such remarkable changes in the appearance and relations of the centrodorsal piece, are I think, partly to be found

in an alteration of the relations of the different surfaces of the radials to one another which takes place during their growth. This is the conclusion to which I have been led by an examination of the separated radials of young and adult examples of *Act. Jukesii;* but it entirely fails to account for the stellate form of the centrodorsal in *Act. stellata* and in *Phanogenia.* This feature appears to me to be a further development of a condition which I have already described in *Act. pectinata;** but I do not expect to get a better understanding of it until I am able to separate the parts of the calyx, and also to make sections through it. Both of these modes of research are at present unavailable, owing to want of material.

The appearances presented by the dorsal surface of an isolated first radial are very different among the different species of *Comatula.* In many *Antedons* such as *Ant. Eschrichtii,* the whole of this surface rests upon the centrodorsal, and except for the edge separating it from the distal articular surface there is no external indication of the presence of a first radial at all, as the second seems to be in direct contact with the centrodorsal. The superior or ventral surface of the latter slopes downwards from its circumference towards the centre.

In such species as *Ant. macrocnema,* however, and in most *Actinometræ* a dorsal view of a first radial shows two surfaces inclined to one another more or less obtusely. One of these appears externally and is the true or outer dorsal surface of the radial. It is often marked by a median dark line which extends outwards over the other radials far on to the arms. The other, or inner dorsal surface, is the surface of synostosis with the centrodorsal plate, and may be at right angles to the outer surface when the ventral face of the centrodorsal is perfectly flat as in *Ant. macrocnema.* But in *Actinometra* it is always placed at an obtuse angle to the outer surface, for the ventral face of the centrodorsal on which it rests slopes downwards and outwards from the centre to the circumference. I have examined the separated radials of two specimens of *Act. Jukesii,* one young with a centrodorsal still marked by cirrhus sockets, and the other full grown with a large discoidal centrodorsal within the radial pentagon, and below the level of its outer surface when viewed from its dorsal aspect. There is a considerable difference in the relative sizes of the inner and outer portions of the dorsal surface of the radials in these two cases. The absolute length of the outer dorsal surface seems to increase very little after a certain stage of growth is reached, for it is nearly the same in the large specimen as in the small one, but the inner or synosteal surfaces of the two differ very greatly in size. This surface is not only absolutely, but also relatively larger in the older specimen,

* See cap. vi, sect. 61, of my memoir on *Actinometra,* now in course of publication in the 'Transactions of the Linnean Society."

taking up more than half the whole dorsal face of the radial, while in the younger specimen it occupies much less than half.

The effect of this change in the component parts of the radial pentagon is to give its central synosteal surface a considerable slope inwards and downwards, so that the whole, when viewed from above, has the form of a wide and shallow funnel. The rim of the funnel (outer dorsal surfaces of the radials) is thick in the young specimen, but does not increase with the growth of the interior (inner dorsal surfaces). Consequently, the centrodorsal which forms, as it were, a plug fitting into the funnel, slips farther and farther down into it, until its dorsal surface becomes level with that of the radial pentagon, or even comes to be actually below it. At the same time it loses its few marginal cirrhi, and their sockets become obliterated, so that the whole dorsal surface of the calyx is one uniform plane. *Act. Jukesii* remains permanently in this condition; but there are other species, as we have seen, and notably *Act. stellata*, in which the centrodorsal loses its pentagonal shape, owing to the appearance of more or less deep clefts between its outer edge and the inner edges of the radials.

In *Act. pectinata* the ventral face of the centrodorsal is divided by ridges into five radial areas, corresponding with the five synosteal surfaces of the first radials that rest upon it. These radial areas are occupied by median depressions, which increase somewhat in depth from their peripheral to their central ends. But the synosteal surfaces of the radials do not exhibit corresponding ridges, for they are marked by similar median depressions, which are also deepest at their central ends. When, therefore, the synosteal surface of the radial pentagon and the ventral surface of the centrodorsal are in their normal state of apposition, they are separated from one another along the median lines of the five radials by five cavities or "radial spaces." Those are largest at their blind central ends, and extend in a peripheral direction to open externally by five minute openings, situated round the margin of the small centrodorsal piece, beneath the radial pentagon which rests upon it, and extends considerably beyond it. It seems to me that we have here an explanation of the large openings between the radials and centrodorsal of *Act. stellata* and *Phanogenia*, &c. In *Act. pectinata* these radial spaces end blindly around the central cavity of the radial pentagon, being shut off from it by the thickened inner margin of its synosteal surface. Whether they are also blind in *Act. stellata*, in which they are so very large, or whether they are in communication with the radial diverticula of the cœlom, which are inclosed within the spouts of the rosette, is a point which can only be settled by making a series of sections through the decalcified calyx.

I have elsewhere (*Actinometra*, cap. iv, § 61) drawn attention to the homology of these openings between the radial pentagon and

centrodorsal of *Act. pectinata* with the openings on the outside of the calyx of *Apiocrinus rotundus* and *Ap. obconicus*, which are situated between every pair of continuous basals, and the radials which rest upon them. Other homologues are the radially situated "inter-articular pores" in the upper part of the stem of *Pentacrinus*.

It is worth notice, that all the species in which the centrodorsal exhibits these variations of form are true *Actinometræ*, *i.e.*, they have an eccentric mouth and a terminal comb on the oral pinnules. In Lovèn's *Phanogenia*, however, the mouth is central, and there is a terminal comb to the oral pinnules. It is thus a very singular exception, for I know of no *Antedon* in which the oral pinnules have this terminal comb, nor one in which the centrodorsal has anything like the form which it has in *Phanogenia*.

In fact, I am able to say that the examination of the "Challenger" *Comatulæ* has entirely confirmed the opinions held by Dr. Lütken and myself (*Actinometra*, cap. ii, §§ 14, 15) respecting the distinguishing characters of *Antedon* and *Actinometra*. We both agree in referring forms with a (sub) central mouth, five equal ambulacra, and no terminal comb on the oral pinnules, to *Antedon*. On the other hand, species with an eccentric mouth, a variable number of unequal ambulacra, and a terminal comb to the oral pinnules, belong to *Actinometra*. There are only two specimens in the "Challenger" collection which have an eccentric mouth but no terminal comb. Pourtales' *Comatula meridionalis* appears to be another, but these are only three exceptions out of some sixty species.

It will be seen at once that these characters are of no use in distinguishing the genera of fossil *Comatulæ*. But, as has been hinted above, there are very considerable differences in the shape of the radials and centrodorsal piece in *Antedon* and *Actinometra* respectively, and as these are exactly the parts which are most met with as fossils, the generic determination of a fossil form is almost as easy as that of a recent one, which has given up its disk to produce a *Hyponome*. As I have described these differences very fully in my *Actinometra* memoir (cap. iv, § 41, 51, 54–56), it is not necessary to do more than refer to them here, with the remark that a more extended knowledge of the species of both genera has only strengthened the opinions which I have there expressed.

The same is the case with regard to the so-called "ventral nerve" of *Comatula*, viz., the fibrillar band underlying the epithelium of the ambulacral grooves. I have already shown (*Actinometra*, cap. iii, § 23–26) that, in *Act. polymorpha* and *Act. solaris*, half, or even more than half, of the arms may have neither groove, epithelium, "nerve," nor tentacles, and I have insisted, as strongly as possible, on the important bearing of this fact on the Ludwig-Gegenbaur view that these subepithelial bands constitute *the* nervous system of the Crinoids.

Neither of these two authors has referred to my statements at all, but both have entirely ignored them. I am now able to repeat them, and to give them much greater force. No less than twenty-three out of the forty-eight species of "Challenger" *Actinometræ*, and three species in Semper's collection, have more or fewer grooveless arms. I have cut sections of these arms in two species, and have obtained the same results as with *Act. polymorpha* and *Act. solaris*. The "ventral nerve" and ambulacral epithelium are conspicuous by their absence, while the axial cords in the skeleton, which I also regard as true nerves, give off branches freely in the centre of each arm-joint, as I have already described for other species both of *Actinometra* and of *Antedon*. Two points are noteworthy. In one species, one of the posterior ambulacral grooves stops quite abruptly on the disk, some little way from the arm bases, and the two arms to which it would naturally have gone with its "nerve," tentacles, &c., receive no branches from any of the adjacent grooves to supply the deficiency.

Lastly, in the gigantic Philippine species already referred to as No. 37, there are more than one hundred arms, many of which are grooveless and "nerveless," as I have found by section-cutting. But these abnormal arms are not limited to the posterior part of the body, as is usually the case, for there are several on each radius.

Evidence of this negative character appears to me to be a serious objection to the German view that the subepithelial bands constitute the *only* nervous apparatus of the Crinoids. Ludwig* attacks Lange's opinions as to the Asterid-nerves, on the ground that the structures supposed by Lange to be nerves are not constant, but are absent from the arms of certain species. It is curious, however, that Ludwig is unable to apply this reasoning to his own views respecting the nerves of the Crinoids!

III. "On the Characters of the Pelvis in the Mammalia, and the Conclusions respecting the Origin of Mammals which may be based on them." By Professor HUXLEY, Sec. R.S., Professor of Natural History in the Royal School of Mines. Received February 24, 1879.

[PLATE 8.]

In the course of the following observations upon the typical characters and the modifications of the pelvis in the Mammalia, it will be convenient to refer to certain straight lines, which may be drawn through anatomically definable regions of the pelvis, as *axes*.

* "Beiträge zur Anatomie der Asteriden." "Zeitschr. für Wiss. Zool.," Band xxx, p. 191.

Of these I shall term a longitudinal line traversing the centre of the sacral vertebræ, the *sacral axis* (Plate 8, *S. a.*); a second, drawn along the ilium, dorso-ventrally, through the middle of the sacral articulation and the centre of the acetabulum, will be termed the *iliac axis* (*Il. a.*); a third, passing through the junctions of the pubis and ischium above and below the obturator foramen, will be the *obturator axis* (*Ob. a.*); while a fourth, traversing the union of the ilium, in front with the pubis, and behind with the ischium, will be the *iliopectineal axis* (*Ip. a.*).

The least modified form of mammalian pelvis is to be seen, as might be expected, in the Monotremes, but there is a great difference between *Ornithorhynchus* and *Echidna* in this respect, the former being much less characteristically mammalian than the latter.

In *Ornithorhynchus* (Plate 8, fig. 4), the ilium is remarkably narrow, and the angle between the iliac and the sacral axis is large, so that the ilium is but very slightly inclined backwards. The iliopectineal axis, nearly at right angles with the iliac axis, is inclined to the sacral axis at an acute angle; while the obturator axis is nearly perpendicular to the sacral axis, and the obturator foramen is relatively small. The front margin of the cotyloid end of the pelvis sends off a very strong *pectineal process* (*p. p.*), from the inferior basal part of which a short, obtuse *tuberculum pubis* (*t. p.*) projects. Between this and the symphysis, the base of the marsupial bone (*Ep. p.*) is attached. The ventral rami of the pubes are short and, like those of the ischium, they are united throughout their whole length in a long symphysis, the ischial division of which (*Sy. I.*) is as long as, if not longer than, the pubic division (*Sy. p.*). The cotyloid ramus of each ischium gives off a stout elongated *metischial process* (*m. p.*) backwards.

In *Echidna* (Plate 8, fig. 5), on the other hand, the ilium is much broader; while the iliac axis inclines downwards and backwards, at an acute angle with the sacral axis. The iliopectineal axis being still at right angles with the iliac axis, makes a much larger angle with the sacral axis; and the obturator axis is inclined from above, at an angle of nearly 45° to the sacral axis, downwards and backwards. In fact, the change in the general character of the pelvis seems to result from its ventral elements having been carried backwards and upwards by the backward and upward shifting of that portion of the ilium which lies below the level of its articulation with the sacrum. There are other changes by which the aspect of the pelvis is much altered. The inner wall of the acetabulum is incompletely ossified, but, in other respects, the pelvis makes a considerable approximation towards the ordinary mammalian form. Thus the pectineal process is represented by a less prominent and more elongated ridge; the metischial process widens out into a mere triangular expansion or "tuberosity," of the ischium, and the symphysial union of the ischia is short.

In all other Mammalia (*e.g.*, *Lepus*, Plate 8, fig. 6) the iliac axis forms

as acute, if not a more acute, angle with the sacral axis; the angle between the ilio pectineal axis and the sacral axis more and more approaches a right angle; and that between the sacral axis and the obturator axis becomes more and more acute. The obturator foramen acquires a much larger proportional size. The symphysial union becomes restricted to a greater or less portion of the pubes; or the ventral halves of the ossa innominata may cease to be directly united, even the pubes being far apart in the dry skeleton. The metischial processes are represented by tuberosities, which may extend upwards and unite with anterior caudal vertebræ; and the ilia may remain narrow or become extremely expanded. In all monodelphous Mammalia the marsupial bones disappear.

The distinctive features of the mammalian pelvis have been clearly indicated by Gegenbaur,* who points out that in mammals, in contradistinction from reptiles, "the longitudinal axis of the ilium gradually acquires an oblique direction, from in front and above, backwards and downwards. The part which represents the crista above thus becomes turned forwards, or more or less outwards, with increase of lateral surface; the acetabular part backwards and downwards; hence the ischium retains its original direction in the produced long axis of the ilium and, at the same time, takes up a position in relation to the vertebral column similar to that which obtains in birds. The conditions of this position are, however, to be sought in factors of a totally different nature in mammals from those which produce it in birds; for, in the former, the ischium follows the changed direction of the ilium, whilst in birds, the ilium has nothing to do with the matter, and the ventral elements of the pelvis appear to pass towards the caudal region, independently of the ilium."

On one point, however, I cannot agree with Gegenbaur's conclusions. He is of opinion that the ilium of mammals answers to the post-acetabular part of the ilium of birds, and that "the *crista ossis ilii* of mammals corresponds with the posterior edge of the post-acetabular part of the bird's ilium. Between the two parts, therefore, there is the difference of a rotation through an angle of almost 180°." On the contrary, it appears to me evident that the whole *crista ilii* in a mammal corresponds with the whole dorsal edge of the ilium in a bird or a reptile, and that the angle through which the iliac axis rotates amounts to not more than 90° (compare Plate 8, fig. 6, *Lepus*, with fig. 9, *Apteryx*). I cannot reconcile the contrary view either with the relations of the ilium to the sacrum, or with the attachment of the muscles.

On comparing the pelvis of *Ornithorhynchus* with that of a lizard

* "Beiträge zur Kenntniss des Beckens der Vögel," "Jenaische Zeitschrift," vi.

(Plate 8, fig. 2), or that of a Chelonian, it will be observed that the resemblance between the former and the Sauropsidan pelvis is, in most respects, closer than that which it bears to the higher Mammalian pelvis. In the reptiles both the pubes and the ischia unite in a ventral symphysis; the pubis has a strong pectineal process, which acquires very large dimensions in the *Chelonia;* the metischial processes are also often very strong. Nevertheless, there is an important difference, for, in all these animals, the iliac axis is either nearly perpendicular to the sacral axis, or slopes from above downwards and forwards; the obturator axis also inclines downwards and forwards. Hence, in most *Lacertilia* and *Chelonia*, the pubes slope forwards very obliquely, while the ischia come more and more forwards. In other words, such modifications of the pelvis as occur in the Lacertilia and the Chelonia are of an opposite kind to those which take place in Mammalia.

The same thing is true of the *Crocodilia* (Plate 8, fig. 3). Here the ilium is much broader than in the lizards and the *Chelonia.* This broadening is effected by the expansion of the ilium, both in front of and behind the iliac axis, which retains about the same inclination to the sacral axis that it has in lizards. The ischia have but small metischial processes, and their long axes lie further forwards than in most lizards. The obturator axis inclines forwards, and the iliopectineal axis is parallel with the sacral axis, as in lizards. As in *Echidna*, a space of the inner wall of the acetabulum is fibrous. The lower boundary of this space is constituted by a prolongation of the anterior end of the cotyloid extremity of the ischium. The interval between this and the anterior end of the ilium answers to the cotyloid end of the pubis in a lizard, but it does not ossify. The pubis corresponds exactly in direction with that of a lizard, but its form is very different. At first narrow and rounded, it gradually flattens from above downwards and, at the same time, widens into a broad trowel-shaped plate of cartilage enclosed in a dense fibrous perichondrium, which lies close beside the middle line in the ventral wall of the abdomen (Plate 8, fig. 12). Each of these flat cartilages is distinct from its fellow throughout the greater part of its extent; but, posteriorly, the two approach, and are united by a broad and strong ligamentous band (*Sy. p. l.*). The bony portion of the pubis commences just outside the acetabulum and extends to this band, terminating by a curved edge directed inwards and forwards. It is the osseous portions of the pubes which are commonly described as the entire pubes of the Crocodilia, and much speculative ingenuity has been expended upon the interpretation* of these apparently anomalous elements of the pelvis, which

* For the latest of these interpretations see Hoffman's excellent memoir, "Beiträge zur Kenntniss des Beckens der Amphibien und Reptilien." "Nied. Archiv für Zoologie," 1876. I cannot but think that had Professor Hoffman studied the crocodile's pelvis in fresh or spirit specimens, he would not have put forward the hypothesis that the pubes of the crocodiles are "epipubes." Rathke's account of

are readily moveable upon their fibro-cartilaginous connexions with the acetabulum. But in no essential respect do they differ from ordinary pubes. Throughout their whole length they give attachment to a muscle, which answers to the pectineus and short adductors of the thigh, while the aponeurosis which lies between them and the ischia gives origin as usual to the obturator externus; and the obturator nerves pass out close to the cotyloid ends of the pubes (Plate 8, fig. 12, *Ob. n.*). For the trowel-shaped forward continuations of the pubes on each side of the symphysis, I will adopt the name of *epipubes*, proposed by Hoffman for other structures which I believe to be homologous with them. They are firmly connected with the aponeurosis of the external oblique muscle, in which, just in front of their outer edges, lie, on each side, the first set of abdominal false ribs (Plate 8, fig. 12, *r*). In short, in all their most important relations, these appear to me to be structures homologous with the marsupial bones of the Monotremes and Marsupials, which in *Thylacinus* are represented by mere cartilages. But although homologous, they are very different in detail; and, in all other respects, the Crocodilian pelvis departs even further than that of the Chelonia and Lacertilia from the Mammalian type.

The *Pterosauria* seem to have possessed epipubes; and in the *Dicynodontia* there is an approximation to the backward elongation of the subsacral part of the ilium which is characteristic of Mammals; but, in both these groups there appears to have been no obturator fontanelle.

In the ornithoscelidan reptiles and in birds (Plate 8, figs. 7, 8, and 9), the pelvis, starting from the lacertilian and crocodilian type, undergoes a series of modifications of a new character, the ultimate result of which is a pelvis as much specialized as that of the higher Mammals, but totally different from it in principle.

The broadening of the ilium seen in the crocodile increases, so that the antero-posterior length of the bone eventually becomes very great, chiefly in consequence of the elongation of the præaxial region of the ilium. But, with all this, the direction of the iliac axis does not sensibly change, and it remains more or less inclined downwards and forwards (Plate 8, fig. 9). The inner wall of the acetabulum is largely membranous. The iliopectineal axis becomes slightly inclined to the sacral axis, but never so much even as in *Echidna*. The main change in the pelvis is, in fact, effected by the extraordinary elongation of the pubes and the ischia, and their rotation backwards and upwards; while, at the same time, the symphysial union of the bones of opposite sides altogether disappears. In *Rhea*, the ischia unite with some of the post-sacral vertebræ as they do in many Mammalia. The pubis becomes very slender and, as it lies parallel with the ischium, the obturator space is

the development of the crocodile's pelvis affords conclusive evidence respecting the homologies of the pubes in these animals.

reduced to a mere slit, often bridged over by a process of the ischium.* The pectineal process is immensely elongated in some Ornithoscelida (as Hulke has shown in *Iguanodon*, and Marsh in *Laosaurus* (Plate 8, fig. 8)); but, in birds, it is usually short (fig. 9), and may be absent, and no epipubes have been discovered, either in the Ornithoscelida or in birds.†

Thus, it appears to be useless to attempt to seek among any known Sauropsida for the kind of pelvis which analogy leads us to expect among those vertebrated animals which immediately preceded the lowest known Mammalia. For, if we prolong the series of observed modifications of the pelvis in this group backwards, the "Promammalia" antecedent to the Monotremes may be expected to have the iliac and obturator axes perpendicular to the sacral axis, and the iliopectineal axis parallel with it; something, in short, between the pelvis of an *Ornithorhynchus* and that of a land tortoise; and provided, like the former, with large epipubes intermediate in character between those of the lower mammals and those of crocodiles. In fact, we are led to the construction of a common type of pelvis, whence all the modifications known to occur in the Sauropsida and in the Mammalia may have diverged.

It is a well-known peculiarity of the urodele Amphibia, that each *os innominatum* consists of a continuous cartilage, the ventral half of which is perforated by a foramen for the obturator nerve, but has no large fibrous fontanelle, or obturator foramen, in the ordinary sense of the word. At the junction of the dorsal with the ventral moiety, the acetabulum marks off the iliac portion of the pelvic arch above, from the pubic and ischial regions below; and these are further distinguishable, even apart from their ossifications, by the position of the foramen for the obturator nerve and the origins of the muscles. In full-grown specimens of *Salamandra maculosa*, the pelvis presents the following characters (Plate 8, figs. 1, 10, and 11):—The iliac axis is slightly inclined forwards, while the iliopectineal axis is practically parallel with the sacral axis. The iliac ossification extends into the acetabulum, and forms a triangular segment of its roof with the apex downwards, exactly as in lizards. The posterior and inferior side of the triangle is separated by a thin band of the primitive cartilage from the upper edge of the similarly triangular cotyloid end of the ischial ossification, the anterior edge of which is vertical, again as in lizards. Between this edge and the anterior and inferior edge of the iliac ossification there is a cartilagi-

* I was, at one time, inclined to think that this represented the union of the pubes and ischia of the same side in ordinary *Sauropsida*, and that the rest of the ischium represented an unusually elongated metischial process; but the study of the development of the pelvis in the chick has convinced me that this is not the case.

† See, however, the observations of Mr. Garrod on a "marsupial bone" in ostriches. "Proceedings of the Zoological Society," 1872.

nous interspace, as in crocodiles, which represents the cotyloid end of the pubis (Plate 8, fig. 1). This cartilaginous part of the pubis gives rise to a pectineal process (*p. p.*), which has the same position as in birds and in *Ornithorhynchus*. In the floor of the acetabulum, the pubic ossification (fig. 1, *Pb.*) makes its appearance as a very thin lamina, which extends, underneath the pectineal process, inwards; and gradually surrounds the whole of the thickened transverse ridge of cartilage which corresponds with the pubis. The pubis is thus represented by an axis of cartilage surrounded by bone, and the thick inner extremities of the two pubes are largely united by fibrous tissue (fig. 10, *Sy. p.*). The ischia are relatively large, and are united, partly by cartilage and partly by ligament, in a long symphysis (fig. 10, *Sy. I.*). Their posterior and external angles are produced into short metischial processes. In one specimen, I observed a distinct sutural line (Plate 8, fig. 11, *s*), between the anterior curved edge of the right ischium and the corresponding pubis, while no such suture could be traced upon the other side.

The pelvic arch of *Salamandra*, therefore, contains all the elements which are found in the higher Vertebrata, but the obturator fontanelle is wanting.

Proceeding from the symphysis pubis, with which it is connected by ligament, is the "ypsiloid" cartilage or epipubis (figs. 1, 11, *Ep. p.*), which was called by the accurate and acute Dugès the "marsupial cartilage." This indication of the homology of the part has been adopted by Cuvier and others, but I do not know that it has been generally accepted. I believe, however, that the identification is perfectly just.

The ypsiloid cartilage proceeds forwards in the middle line, as a stem (*Ep. p.*) of variable length, and then divides into two branches (*Ep. p.*^{1} *Ep. p.*^{2}), which diverge at right angles to one another, and terminate by rounded extremities. The pedicle of the ypsiloid cartilage is broad and triangular in section, the apex of the triangle being dorsal.

The manner in which the abdominal muscles are connected with this cartilage is very instructive (Plate 8, figs. 13, 15). The anterior pectinations of the external oblique muscle (*O. e.*) are inserted into a thin but dense fascia, which is hardly separable from the superjacent dermis, and which extends across the middle line to the muscle of the other side. The most posterior bundles of the external oblique, however, are inserted by a rounded tendon into the pectineal process (Plate 8, fig. 15, *p.p.*), which therefore answers to the spine or tuberosity of the pubis in Mammalia. Internal to this, the fascia arches over a small space (*i.r.*) (which corresponds with the inguinal ring), and is then inserted into the whole length of the pedicle and into the posterior half of each ramus of the ypsiloid cartilage. Beyond this point the rami are free externally, though they lie close against the fascia. On their inner sides, however,

they are connected together and with the fascia of the external oblique, in the middle line, by a delicate sheet of fibrous tissue (figs. 13 and 15, *f*). On comparing this disposition of the tendon of the external oblique muscle with that which obtains in *Ornithorhynchus* (Plate 8, fig. 14) the correspondence is obvious. For, in the latter, the posterior fibres of that muscle are inserted directly into the spine of the pubis (*t. p.*). Between these and the outer edge of the marsupial bone there is fibrous interspace, corresponding with the inguinal ring (*i. r.*); while the more anterior fibres of the external oblique are inserted into the apex of the bone, which is, as it were, imbedded in the fascia. If the pedicle of the ypsiloid cartilage were reduced, the rami at the same time widening behind, until their outer angles reached the pectineal processes, and their free apices shortening, the ypsiloid cartilage of the salamander would be converted into two cartilages having exactly the same relation to the tendon of the external oblique that the marsupial bones have in the *Ornithorhynchus*.

In the Monotremes (Plate 8, fig. 14, *Py.*), there are two very large pyramidales muscles, which spring from the whole inner margins of the marsupial bones; their posterior and middle fibres run to the middle line, but the anterior ones constitute a longitudinal band, which extends forwards, and is inserted along with the rectus (*R.*), of which, indeed, it looks like a part. In the salamander, muscular fibres similarly take their origin from the inner edges of the rami of the epipubis; and the most anterior of these fibres pass forwards, and become more or less confounded with the inner edges of the recti (fig. 13, *Py.*). But the region which answers to the *linea alba* (*l. a.*) is very broad, whence the pyramidales are separated by a wide interval.

Thus far, the muscles which are connected with the epipubis are strictly comparable with those which are attached to the marsupial bones. But, in *Salamandra*, there is a muscle (Plate 8, fig. 13, *A.*), of which I have been able to find no representative in the Monotremes I have dissected. This is a thin band of longitudinal fibres, which spring partly from the pubis, close to the symphysis, and partly from the outer edge of the pedicle of the epipubis, and are inserted into the outer edge of the ramus (Plate 8, fig. 13, *A.*). External to this, a broad flat band of muscular fibres (fig. 13, *B.*) takes its origin from the pubis and its pectineal process, and runs forwards to be inserted into the modified branchial arches and the tongue. These are the *hebosteoglossi* of von Siebold ("Observationes quædam de Salamandris et Tritonibus"). Superficial to this muscle, is the proper rectus (*R.*) itself, marked by its tendinous intersections; while, on the dorsal or deep side of both, there is a curious fan-shaped muscle (*C.*), which takes its origin from the pectineal process, by a narrow tendon, and spreads out to be inserted into the outer face of the pedicle and the outer edge of the ramus of its side. The ventral face of the pedicle

and the posterior halves of the rami are closely adherent to the fascia of the external oblique in the middle line, and lie between the inner edges of the two recti. Finally, the fibres of the transversus (fig. 13, *Trs.*) are inserted into the outer edges of the rami, and the fascia which unites these appears to belong to the transversus.

On comparing the recti and the muscles A, B, C, with those of *Ornithorhynchus* and *Echidna*, a great apparent difference manifests itself. For the very thin recti of the Monotremes take their origin from the pubis along a line which extends from the tubercle to close to the symphysis, and pass forwards, dorsad of the marsupial bones and the pyramidales, which thus lie altogether in front of them and are, by them, largely separated from the fascia transversalis. Nevertheless, it will be observed that the origin of the muscle B nearly corresponds with that of the rectus in *Ornithorynchus;* and I am disposed to think that, in this animal, the rectus, at least in its posterior moiety, is represented by the homologue of this muscle, which has extended laterally over the dorsal face of the enormously enlarged homologues of the rami of the ypsiloid cartilage.

However this may be, it must be recollected that it is only the extreme ends of the rami which lie dorsad of the recti, and that, in the rest of its extent, the epipubis of *Salamandra* is firmly fixed to the fascia of the external oblique, which forms the front wall of the sheath of the rectus. The homology of the epipubis with the marsupial bones is determined by the essential identity of the relations of the two to the tendons of the external oblique muscles.

It seems to me that, in such a pelvis as that of *Salamandra*, we have an adequate representation of the type from which all the different modifications which we find in the higher Vertebrata may have taken their origin.

In the lizards and the *Chelonia* the iliac and obturator axes have inclined forwards, and the epipubes have been reduced to such rudiments, as have been described in chameleons and in some tortoises.*

In the crocodiles, with the same general pelvic characters, the cotyloid end of the pubis retains its imperfectly ossified condition, while the epibubes represent the vastly enlarged rami of the salamandrine epipubis.

In the *Ornithoscelida* and in birds, the ilia elongate, but it is the modification of the pubes and ischia which is the most characteristic feature of the pelvis, and the epipubis vanishes.

In the *Pterosauria* and in the Dicynodonts, the salamandrine non-development of an obturator fontanelle persists; and, in the former, the sessile rami of the epipubis appear to be represented by the so-called marsupial bones.

* Hoffmann, "Beiträge zur Kenntniss des Beckens der Amphibien und Reptilien," "Nied. Archiv für Zoologie," Bd. 3, p. 143, 1876.

Unless the like should prove to be the case in the Dicynodonts, it is in the Mammalia alone that the subsacral portion of the ilium elongates backwards, carrying with it the pubis and the ischium, between which a large rounded obturator fontanelle is developed.

These facts appear to me to point to the conclusion that the Mammalia have been connected with the Amphibia by some unknown "promammalian" group, and not by any of the known forms of Sauropsida; and there is other evidence which tends in the same direction.

Thus, the Amphibia are the only air-breathing Vertebrata which, like Mammals, have a dicondylian skull. It is only in them that the articular element of the mandibular arch remains cartilaginous; while the quadrate ossification is small, and the squamosal extends down over it to the osseous elements of the mandible; thus affording an easy transition to the mammalian condition of these parts.

The pectoral arch of the Monotremes is as much amphibian as it is sauropsidan; the carpus and the tarsus of all Sauropsida, except the *Chelonia*, are modified away from the Urodele type, while those of the Mammal are directly reducible to it; and it is perhaps worth notice, that the calcar of the frogs is, in some respects, comparable with the spur of the Monotremes.

Finally, the fact that, in all Sauropsida, it is a right aortic arch which is the main conduit of arterial blood leaving the heart, while, in Mammals, it is a left aortic arch which performs this office, is a great stumbling-block in the way of the derivation of the Mammalia from any of the Sauropsida. But, if we suppose the earliest forms of both the Mammalia and the Sauropsida to have had a common Amphibian origin, there is no difficulty in the supposition that, from the first, it was a left aortic arch in the one series, and the corresponding right aortic arch in the other, which became the predominant feeder of the arterial system.

The discovery of the intermediate links between Reptilia and Aves, among extinct forms of life, gives every ground for hoping that, before long, the transition between the lowest Mammalia at present known and the simpler Vertebrata may be similarly traced. The preceding remarks are intended to direct attention to the indications of the characters of these promammalian Vertebrata, which the evidence at present forthcoming seems to me to suggest.

In the relatively large size of the brain, and in the absence of teeth, the only existing representatives of the *Ornithodelphia* present characters which suggest that they are much modified members of the group. On comparing the brain of *Echidna*, for example, with that of many *Marsupialia* and *Insectivora*, its relative magnitude is remarkable: and, in view of the evidence which is now accumulating, that the brain increases in size in the later members of the same series of Mammalia, one may surmise that *Echidna* is the last

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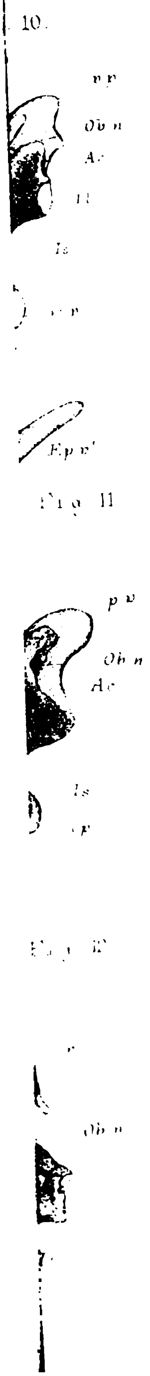


Fig. 13.

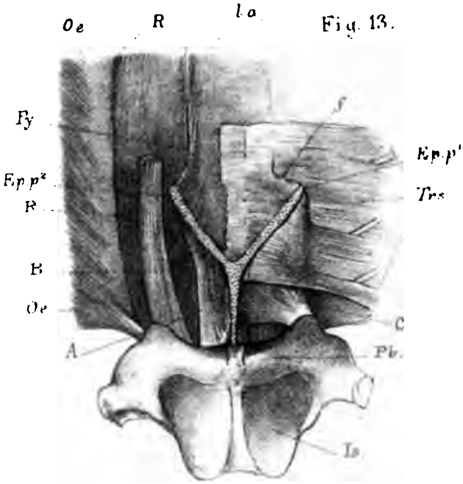


Fig. 14.

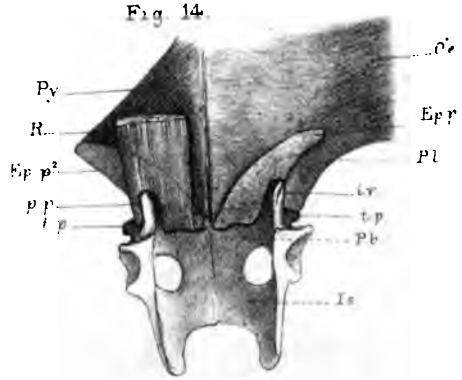
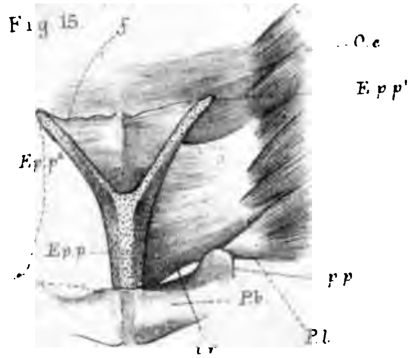


Fig. 15.



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term of a series of smaller-brained *Ornithodelphia*. Among the higher Vertebrata, I think that there is strong reason to believe that edentulous animals are always modifications of toothed forms.

EXPLANATION OF PLATE 8.

Figs. 1 to 9. The left half of the pubic arch in *Salamandra* (fig. 1), *Iguana* (fig. 2), *Crocodilus* (fig. 3), *Ornithorhynchus* (fig. 4), *Echidna* (fig. 5), *Lepus* (fig. 6), *Compsognathus* (fig. 7), *Laosaurus* (fig. 8), and *Apteryx* (fig. 9). The letters have the same signification throughout. *Il.* ilium, *Pb.* pubis, *Is.* ischium, *Ep.p.* epipubis, *S. a.* sacral axis, *Ip. a.* ilio-pectineal axis, *Ob. a.* obturator axis, *Il. a.* iliac axis, *Sy. p.*, *Sy. I.*, indicate the extent to which the pubes and the ischia unite respectively in their ventral symphyses; *p. p.* pectineal process, *t. p.* tuberosity or spine of the pubis, *m. p.* metischial process or tuberosity of the ischium, *Cl.* os cloacæ in *Iguana*.

Figs. 10, 11. Dorsal and ventral views of the ventral half of the pelvis of *Salamandra maculosa* (× 4). The letters as before, except *Epp.*[1] *Epp.*[2], right and left rami of the epipubis or ypsiloid cartilage, *Ob. n.* foramen of the obturator nerve, *s.* trace of a suture between the ischium and the pubis on the right side, *Ac.* acetabulum.

Fig. 12. Ventral aspect of the pelvis of a small *Crocodilus acutus* and the hindermost abdominal false ribs, of the natural size. The obturator nerve (*Ob. n.*) perforates the aponeurosis, which fills up the rhomboidal space between the ischia and the pubes, on the inner side of the pubis. *Sy. p. l.* the ligamentous union of the pubes.

Fig. 13. Dorsal aspect of the epipubis and the muscles connected with it in *Salamandra maculosa*. On the left side, the transversalis and the muscle *C.* are removed.

Fig. 15. The same, all the muscles but the external oblique being removed.

Fig. 14. The epipubis and the adjacent muscles in *Ornithorhynchus*.

R. rectus abdominus, *Py.* pyramidalis, *O. e.* obliquus externus, *Trs.* transversus, *l. a.* linea alba, *f.* fascia, extending between the two rami of the epipubis in *Salamandra*, *P.l.* the representative of Poupart's ligament, *i. r.* inguinal ring, *A. B. C.* muscles of Salamandra described in the text. *A.* and *C.* together appear to be the *pubio-marsupial* of Dugès: *B.* is the *hebosteoglossus* of von Siebold.

March 13, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Influence of Electricity on Colliding Water Drops." By Lord RAYLEIGH, F.R.S. Received February 27, 1879.

It has been known for many years that electricity has an extraordinary influence upon the behaviour of fine jets of water ascending in a nearly vertical direction. In its normal state a jet resolves itself into drops, which even before passing the summit, and still more after passing it, are scattered through a considerable width. When a feebly electrified body is brought into its neighbourhood, the jet undergoes a remarkable transformation, and appears to become coherent; but under more powerful electrical action the scattering becomes even greater than at first. The second effect is readily attributed to the mutual repulsion of the electrified drops, but the action of feeble electricity in producing apparent coherence has been a mystery hitherto.

It has been shown by Beetz that the coherence is apparent only, and that the place where the jet breaks into drops is not perceptibly shifted by the electricity. By screening various parts with metallic plates, Beetz further proved that, contrary to the opinion of earlier observers, the seat of sensitiveness is not at the root of the jet where it leaves the orifice, but at the place of resolution into drops. As in Sir W. Thomson's water-dropping apparatus for atmospheric electricity, the drops carry away with them an electric charge, which may be collected by receiving the water in an insulated vessel.

I have lately succeeded in proving that the normal scattering of a nearly vertical jet is due to the *rebound* of the drops when they come into collision with one another. Such collisions are inevitable in consequence of the different velocities acquired by the drops under the action of the capillary force, as they break away irregularly from the continuous portion of the jet. Even when the resolution is regularised by the action of external vibrations of suitable frequency, as in the beautiful experiments of Savart and Plateau, the drops must still come into contact before they reach the summit of their parabolic path. In the case of a continuous jet the "equation of continuity" shows that as the jet loses velocity in ascending, it must increase in section. When the stream consists of drops following the same path in single file, no such increase of section is possible, and then the constancy of the total stream requires a gradual approximation of the drops, which in the case of a nearly vertical direction of motion cannot stop short of actual contact. Regular vibration has, however, the effect of postponing the collisions and consequent scattering of the drops, and in the case of a direction of motion less nearly vertical may prevent them altogether.

Under moderate electrical influence there is no material change in the resolution into drops, nor in the subsequent motion of the drops up to the moment of collision. The difference begins here. Instead of rebounding after collision, as the unelectrified drops of clean water generally or always do, the electrified drops *coalesce*, and thus the jet is no longer scattered about. When the electrical influence is more powerful, the repulsion between the drops is sufficient to prevent actual contact, and then of course there is no opportunity for amalgamation.

These experiments may be repeated with extreme ease and with hardly any apparatus. The diameter of the jet may be about $\frac{1}{20}$ inch, and may be obtained either from a hole in a thin plate or from a drawn-out glass tube. I have generally employed a piece of glass tube fitted at the end with a perforated tin plate, and connected with a tap by india-rubber tubing. The pressure may be such as to cause the jet to rise 18 or 24 inches, or even more. A single passage of a rod of gutta-percha, or of sealing-wax, along the sleeve of the coat is sufficient to produce the effect. The seat of sensitiveness may be investigated by exciting the extreme tip only of a glass rod, which is then held in succession to the root of the jet and to the place of resolution into drops. An effect is observed in the latter but not in the former position. Care must be taken to use an electrification so feeble as to require close proximity for its operation, otherwise the discrimination of the positions will not be distinct.

The behaviour of the colliding drops becomes apparent under instantaneous illumination. I have employed sparks from an inductorium, whose secondary terminals were connected with the coatings of a Leyden jar. The jet should be situated between the sparks and the eye, and the observation is facilitated by a piece of ground glass held a little beyond the jet, so as to diffuse the light; or the *shadow* of the jet may be received on the ground glass, which is then held as close as possible on the side towards the observer.

If the jet be supplied from an insulated vessel, the coalescence of colliding drops continues for a time after the removal of the influencing body. This is a consequence of the electrification of the vessel. If the electrified body be held for a time pretty close to the jet, and be then gradually withdrawn, a point may be found where the rebound of colliding drops is re-established. A small motion *to* or *from* the jet, or a discharge of the vessel by contact of the finger, again induces coalescence.

Although in these experiments the charges on the colliding drops are undoubtedly of the same name, it appeared to me very improbable that the result of contact of two equal drops, situated in the open, could be affected by any strictly equal electrifications. At the same time an opposite opinion makes the phenomena turn upon the very small

differences of electrification due either to irregularities in the drops or to differences of situation, and is at first difficult of acceptance in view of the efficiency of such very feeble electric forces. Fortunately I am able to bring forward additional evidence bearing upon this point.

When two horizontal jets issue from neighbouring holes in a thin plate, they come into collision for a reason that I need not now stop to explain, and after contact they frequently rebound from one another without amalgamation. This observation, which I suppose must have been made before, allowed me to investigate the effect of a passage of electricity across two contiguous water surfaces. The jets that I employed were of about $\frac{1}{16}$ inch in diameter, and issued under a moderate pressure (5 or 6 inches) from a large stoneware vessel. Below the place of rebound, but above that of resolution into drops, was placed a piece of insulated tin plate in connexion with a length of gutta-percha-covered wire. The source of electricity was a very feebly excited electrophorous, whose cover was brought into contact with the free end of the insulated wire. When both jets played upon the tin plate, the contact of the electrified cover had no effect in determining the union, but when only one jet washed the plate, union instantly followed the communication of electricity, and this notwithstanding that the jets were already in communication through the vessel. The quantity of electricity required is so small that the cover would act three or even four times without being re-charged, although no precautions were taken to insulate the reservoir.

In subsequent experiments the colliding jets, about $\frac{8}{100}$ inch in diameter, issued horizontally from similar glass nozzles, formed by drawing out a piece of glass tubing and dividing it with a file at the narrowest part. One jet was supplied from the tap, and the other from the stoneware bottle placed upon an insulating stool. The sensitiveness to electricity was extraordinary. A piece of rubbed gutta-percha brought near the insulated bottle at once determined the coalescence of the jets. The influencing body being held still, it was possible to cause the jets again to rebound from one another, and then a small motion of the influencing body *to* or *from* the bottle again induced coalescence, but a *lateral* motion without effect. If an insulated wire be in connexion with the contents of the bottle, similar effects are produced when the electrified body is moved in the neighbourhood of the free end of the wire. With care it is possible to bring the electrified body into the neighbourhood of the free end of the wire so *slowly* that no effect is produced; a sudden movement of withdrawal will then usually determine the coalescence.

Hitherto statical electricity has been spoken of; but the electromotive force of even a single Grove cell is sufficient to produce these phenomena, though not with the same certainty. For this purpose one pole is connected through a contact key with the interior of the

stoneware bottle, the other pole being to earth. If the fingers be slightly moistened, the body may be thrown into the circuit, apparently without diminution of effect. This perhaps ought not to surprise us, as in any case the electricity has to traverse several inches of a fine column of water. On the other hand, it appeared that most of the electromotive force of the Grove cell was necessary.

Further experiment showed that even the discharge of a condenser charged by a single Grove cell was sufficient to determine coalescence. Two condensers were used successively; one belonging to an inductorium by Ladd, the other made by Elliott Brothers, and marked "Capacity $\frac{1}{2}$ Farad." Sometimes even the "residual charge" sufficed.

It must be understood that coalescence of the jets would sometimes occur in a capricious manner, without the action of electricity or other apparent cause. I have reason to believe that some, at any rate, of these irregularities depended upon a want of cleanness in the water. The addition to the water of a very small quantity of soap makes the rebound of the jets impossible.

The last observation led me to examine the behaviour of a fine vertical jet of slightly soapy water; and I found, as I had expected, that *no scattering took place.* Under these circumstances the approach of a moderately electrified body is without effect, but a more powerful influence scatters the drop as usual. The apparent coherence of a jet of water when the orifice is oiled was observed by Fuchs, and appears to have been always attributed to a diminution of adhesion between the jet and the walls of the orifice.

Some further details on this subject, and other investigations respecting the phenomena of jets, are reserved for another communication, which I hope soon to be able to present to the Royal Society; but I cannot close without indicating the probable application to meteorology of the facts already mentioned. It is obvious that the formation of rain must depend very materially upon the consequences of encounters between cloud particles. If encounters do not lead to contacts, or if contacts result in rebounds, the particles remain of the same size as before; but, if the issue be coalescence, the bigger drops must rapidly increase in size and be precipitated as rain. Now, from what has appeared above we have every reason to suppose that the results of an encounter will be different according to the electrical condition of the particles, and we may thus anticipate an explanation of the remarkable but hitherto mysterious connexion between rain and electrical manifestations.

II. "On the Influence of Coal-dust in Colliery Explosions." No. 2. By W. GALLOWAY. Communicated by ROBERT H. SCOTT, F.R.S., Secretary to the Council of the Meteorological Office. Received February 27, 1879.

In the former communication on this subject, which I had the honour of submitting to the Fellows ("Proc. Roy. Soc.," vol. xxiv, p. 354), some experiments were described which showed that a mixture of air and coal-dust of a certain known chemical composition was not inflammable at ordinary pressure and temperature; and that, when 0·892 per cent. of fire-damp (by volume), or a greater proportion, was added to the same mixture, it became inflammable and burned freely with a red smoky flame. The general conclusion to which the second result pointed was also stated in the same place to be, that, an explosion originated in any way whatever in a dry and dusty mine, may extend itself to remote parts of the workings, where the presence of fire-damp was quite unsuspected.

The wetness or dryness of the workings of a mine depends, other things being equal, on the temperature of the strata in which they are situated: for it is obvious that if, on the one hand, the temperature of the mine is lower than the dew-point of the air at the surface, the ventilating current will deposit moisture as it becomes cooled in passing through the workings; and if, on the other hand, the temperature of the mine is higher than the dew-point of the air at the surface, the ventilating current will absorb moisture and tend to produce a state of dryness. It is well known, however, that the temperature of the strata in the Coal Measures of this country increases at the rate of about 1° F. for every 60 feet of additional depth below the surface, and, therefore, from what precedes, it is evident *that the comparative wetness or dryness of a mine depends on its depth.*

As far as my own observations are concerned, I have found that coal mines, shallower than 400 feet, are damp or wet, and those deeper than 700 feet are dry and dusty: between these two points, also, there appears to be a kind of debateable ground in which wetness or dryness depends, for the time being, on the coldness or warmness of the air entering the mine at the surface.

In all dry coal mines the coal-dust lying on the floor of the roadways rises in clouds and fills the air when it is disturbed by the passage of men, horses, small waggons, &c.; a sudden puff of air, therefore, such as that produced by a local explosion of fire-damp, or by a shot blowing out its tamping, must necessarily produce the same effect in a greater or less degree according to its intensity. The mixture of coal-dust and air, formed by the action of either the fire-

damp explosion or the blown-out shot, will be inflammable if it contain any larger proportion of fire-damp than 0·892 per cent., and the flame of the original explosion will pass on through it, extending the area of the disturbance as far as the same conditions exist, or, it may be, to the utmost limits of the workings. If it contain more than 0·892 per cent. of fire-damp, it will be more and more explosive, according as the proportion of fire-damp is greater, until a maximum point is reached, beyond which its explosiveness will begin again to decline. If, lastly, it contain less than 0·892 per cent. of fire-damp, or even if it consist only of coal-dust and pure air, it will still be so nearly inflammable that it will probably become so when it undergoes the compression and consequent heating which the occurrence of an explosion in one part of a confined space must necessarily produce throughout the remainder of the same space. It is probable, moreover, that some kinds of coal-dust require less fire-damp than others to render their mixture with air inflammable; and it is conceivable that still other kinds may form inflammable mixtures with pure air.

I have partially investigated the relation between the proportions of air, coal-dust, and gas* required to insure inflammation or explosion on the application of a light; but as the series of experiments is not yet complete, I propose to reserve their description for some future opportunity. I may mention, however, that in the apparatus which I have hitherto employed, the proportion of coal-dust which gave the best results was much larger than might at first sight be thought necessary, namely, about one ounce of dust to a cubic foot of air for all mixtures of gas and air, ranging between one of gas and twenty of air, and one of gas and forty of air. Also, in one of the experiments with the return air of a mine, which I propose to describe in this place, the air requires to be literally *black with dust* before it will ignite. It is, therefore, obvious that the particles which are floating about in the air of a dry mine, in its normal state, cannot render it inflammable; and it is probable that only the sweeping action of a gust of wind, like a squall, passing along the galleries, can raise a sufficient quantity to do so.

Some of the colliery explosions which have occurred during the last two years are amongst the most disastrous on record, and the attempts that have been made to explain them are of the usual unsatisfactory character. The assumption, without a vestige of proof that fire-damp has suddenly burst from the strata, is still maintained even in cases in which the flame is seen to have ramified into the extremity of every *cul-de-sac* and extended to the opposite boundaries of the workings. The very token whereby the ubiquity of the flame is made manifest, is the so-called *charring* of the timber,

* As these experiments were only preliminary ones made chiefly for the purpose of testing the apparatus, common lighting gas was employed in them.

coal, and rubbish; and this, generally in the case of the timber, and always in the case of the coal and rubbish, consists of a coating of coked coal-dust adhering to them superficially, and testifying unmistakably by its presence that coal-dust has actually been playing the part which is claimed for it by myself and others.

Following are a few of the details that have become known regarding the most recent explosions of importance:—

Pemberton (11th October, 1877). 36 men killed. Depth 1,005 feet. At page 333 of the "Reports of the Inspectors of Mines," it is said:—"The Pemberton Colliery had been held up as a model of engineering, and seemed to be the last place at which a disaster of this kind was likely to happen." At page 332 of the same volume, the following gratuitous explanation of the explosion is given:—"The effect of a shot blowing out, and which appears to have occurred, would be to exhaust the face and sides of Rutter's place and Price's place, and this additional fire-damp rushing out into an atmosphere already heavily charged, would bring the air in this particular district up to the explosive point."

Blantyre (22nd October, 1877). 207 men killed. Depth of the workings from 800 to 900 feet. The seam is not very gaseous and the mine was supposed to be well ventilated. It was impossible to say where the explosion began. At page 7 of the official report it is said:—"The explosion extended throughout miles of the workings and was of the most violent kind. The gas in a large portion of the workings had apparently been mixed to a highly explosive state. The noise at the top of No. 2 shaft is described as having been like a shot in a sinking pit, and a great volume of smoke and dust came to the top. On the top of No. 3 shaft the noise was like the bursting of a steam pipe, or shot in a sinking pit, and was as quickly over, flame coming out of the shaft mouth. Flame seems to have extended through nearly all the working places." Again, at page 11:—"The mine being dry and dusty, and the dust being mixed with highly inflammable splint coal, would help to spread the flame and give force to the explosion." Lastly, at page 206 of the notes of evidence, a witness says:—"I desire to make a suggestion. On one occasion Mr. Watson (the manager) told me that the mine, being a dry one, like a desert, the coal-dust would aggravate an explosion."*

Unity Brook (12th March, 1878). 43 men killed. Depth of the workings 792 feet. The workings were examined, and found to be safe, half an hour before the explosion. The mine was dry and dusty.

* It is encouraging to observe that the agency of coal-dust has thus been recognized by some persons connected with mining, although it appears to have entirely escaped the notice of every one except Faraday and Lyell, and some of the French mining engineers, until after the appearance of my first paper on this subject in 1876.

Smoke and soot came up the upcast. The flame had travelled all through the mine and 100 yards up the shaft. Naked lights were used and shots were fired.

Apedale (27th March, 1878). 40 men killed. Depth (?). Smoke and flame came up the shaft. The workings were set on fire.

Haydock (7th June, 1878). 195* men killed. Depth of the workings 750 feet. Smoke and dust were ejected from the shafts. The mine was dry and dusty. It was not possible to say how or where the explosion had occurred. Locked safety lamps were used and no shots were fired in the district where the explosion happened.

Abercarne (11th September, 1878). 264 men killed. Depth (?). A flash of flame and a column of black smoke ascended high into the air above the mouth of the shaft. The workings were set on fire. This mine was well ventilated, and no accumulations of gas of any consequence were known to exist in it. The workings were unusually dry and contained much very fine coal-dust. Locked safety lamps were used and no shots were fired.

Dinas (13th January, 1879). 63 men killed. Depth of the shaft, 1,218 feet. The workings extended under high ground, where they were from 1,500 to 1,800 feet below the surface; they were very dry and dusty. Small accumulations of explosive gas were sometimes formed in them, but not of sufficient magnitude to account for the disaster. The bottom of the principal shaft was filled up with rubbish in consequence of the timber which supported the entrance to the workings being blown away by the explosion. This obstruction has not yet been removed at the time I write, and the workings have not been entered, nor the bodies got out. I had visited this mine several times during the two or three years preceding the accident, and knew its general condition well. The return air coming from the working places, and therefore filling nearly one-half of the existing open space, *contained always more than 2 per cent. of fire-damp.* In this respect it did not materially differ from the return air of most of the steam coal collieries in the district, being better than some and worse than others. If there had been no coal-dust present I should have considered it to be comparatively safe. As it was, I strongly and repeatedly urged the manager to water the roadways so as to keep them always damp or moist, and he actually had two water-carts made for that purpose. On the occasion of my last visit before the explosion, however, I found they were not being employed, and I had no power to enforce my views. The result has been exactly what might have been anticipated, and what is liable to happen any day in every mine similarly circumstanced. It is quite plain that, with 2 per

* The official reports are not yet published, and in some cases the number of men killed may not be quite correct, as they are taken from the reports in the *Times.*

cent. of fire-damp in the return air, the slightest puff of a local fire-damp explosion, or of a blown-out shot, will raise sufficient dust to increase the amount of inflammable matter a hundredfold, and produce all the phenomena that have been observed in this and similar cases. Locked safety lamps were used, and shots were fired. The cause of this explosion, like that of the preceding ones, will in all probability never be ascertained.

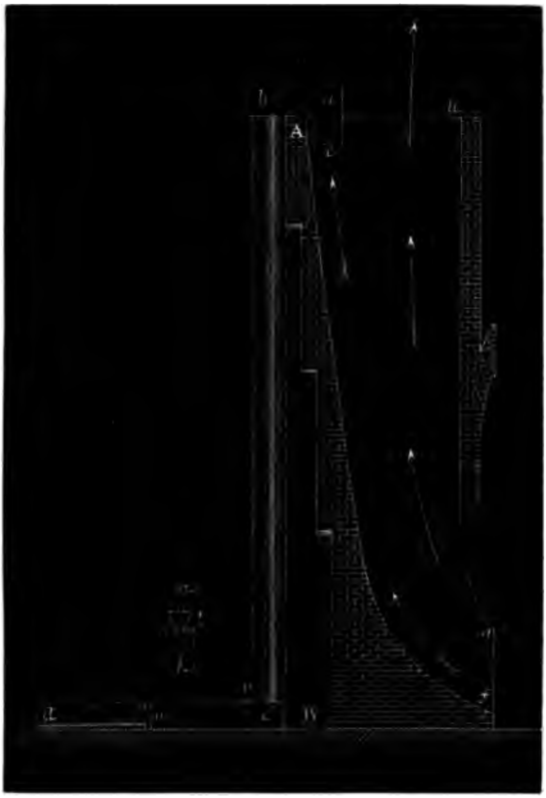
When smoke and soot are produced; or dust is ejected from the shafts; or the coal, stone, and timber have a charred appearance, due to a deposit of coked coal-dust on their surface; or, lastly, when large superficies of the sides of the galleries are found to be on fire immediately after the event, we may safely conclude that coal-dust has played an important, if not a predominant, part in the explosion. The manner in which coal-dust operates in setting fire to coal and timber is probably as follows:—The air is travelling rapidly in one direction along a gallery, throwing a continuous shower of dust, small pieces of coal, &c., against all surfaces that deflect it or obstruct its course; at the instant the flame traverses it, however, the coal-dust is melted; it then assumes the properties of flaming pitch, adheres to the surfaces against which it is thrown, and rapidly accumulates until it forms a crust of greater or less thickness, according to the length of time the air continues to travel in the same direction. If it is thick enough to retain its high temperature, and is supplied with fresh air immediately, it continues to burn, and the flame soon communicates itself to the body of the coal or timber; but if it is thin, or if the surrounding atmosphere cannot support combustion, it becomes extinguished. In the second case, the surface covered with the crust or layer of coke is vulgarly said to be *charred.*

During the course of the past year I have been enabled to make a considerable number of experiments with mixtures of coal-dust, air, and fire-damp; thanks to the liberality of the Lords of the Committee of Council on Education, who acted upon the recommendation of the Government Grant Committee in affording me pecuniary aid; and thanks also to the kind co-operation of Mr. Archibald Hood, managing director of Llwynypia Colliery, and his two sons, Messrs. Robert and William Hood, whose assistance has been quite invaluable to me. The two experiments I propose now to describe were made at Llwynypia Colliery with the coal-dust and fire-damp, whose analyses are given at pp. 357 and 358 of the "Proceedings," No. 168, 1876.

In order to test the truth of the hypothesis that the return air of a mine in which a considerable amount of fire-damp is emitted by the coal may be rendered inflammable by the addition of coal-dust, I had an apparatus constructed at Llwynypia Colliery, and placed close to the ventilating fan in such a position that a current of the return air from the upcast shaft could be made to pass through it at pleasure.

Referring to fig. 1, which represents the whole arrangement, *e*, *f*, *g*, *h* is the chimney of a Guibal fan, through which all the air from the

FIG. 1.



workings, amounting to about 80,000 cubic feet per minute, is ejected into the atmosphere. *a*, *b*, *c*, *d* is a bent pipe partly made up of square wooden boxes, partly of round sheet iron pipes; at the end, *a*, it overhangs, and partly dips into, the chimney of the fan; and at the other end the part *c*, *d*, runs along the surface of the ground. *k* is a branch of the same area in cross section as the other wooden parts of the apparatus; it is covered on the top, but is provided with a hopper *l*, having a wooden plug *m*, through which coal-dust can be introduced, *v* is a valve by means of which the velocity of the current can be regulated, and *n* is a door.

When the regulating valve is full open a strong current of return air, amounting to 1,251 cubic feet per minute, passes through the apparatus, and makes its escape at *d*. This air is not only saturated with

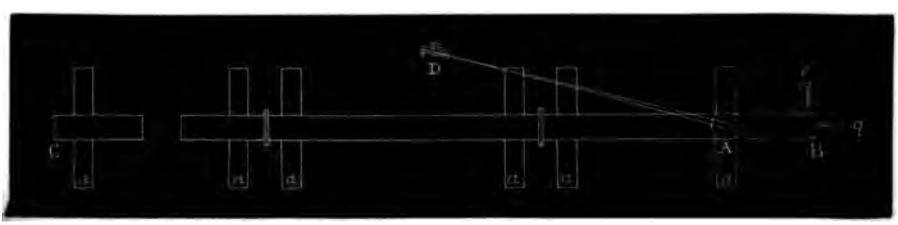
water, but it contains innumerable globules of water floating in it. On the 5th of October last its temperature was 69°·3 F. An oil lamp, having a good large flame, was placed inside the door *n*, so that the flame was in the centre of the current, and it was then found that the temperature of the air had increased to 74°·5. The temperature, quantity, and quality of the various currents of return air in this colliery were, on the 11th of April, 1878, as follows:—

Date upon which the observation was made.	Number of current.	Cubic feet of air per minute.	Temperature Fahr.	Height of cap on small flame.	Approximate percentage of fire-damp in the air.
1878.			Dry.	Inch.	
April 11	1	10,128	66°	$\frac{1}{8}$	2 per cent.
,, ,, ...	2	21,000	74°	$\frac{3}{16}$	2¼ ,,
,, ,, ...	3	44,421	73°	$\frac{1}{4}$	2½ ,,
		75,549			

The elevation of temperature due to placing the lamp inside the apparatus is, therefore, not abnormal. The hopper having been filled with coal-dust the plug was raised somewhat and stirred about so as to determine the entry of dust into the chamber *k*. The immediate result was the appearance of a large and very hot red flame at the mouth of the pipe *d*. The length of the visible part of the flame varied from 6 to 8 feet, and its greatest diameter from 2 to 2½ feet; and it was accompanied by large volumes of black smoke and dust. The pipe *d* soon became so hot that it could not be approached closely.

The second experiment is intended to illustrate the effects of an explosion of fire-damp in a dry mine containing coal-dust. One part of the apparatus represents a gallery with coal-dust lying on its floor as well as on the horizontal timbers, the buildings, and other rough surfaces at its top and sides; another part represents a cavity in the roof containing an explosive mixture of fire-damp and air. When the explosive gas is ignited the flame sweeps down into the gallery, the disturbance raises the coal-dust and the results are exactly those that have been foreseen. Figs. 2 to 6 show all that is necessary for under-

FIG. 2.



[redacted]

FIG. 3.

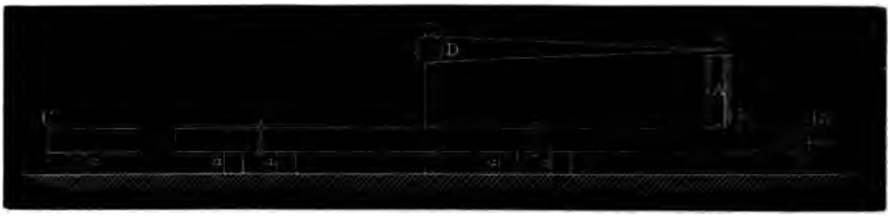


FIG. 4.

FIG. 5.

FIG. 6.

[redacted]

standing the apparatus and the experiment. In all the figures, A represents the cavity in the roof: it is a galvanized sheet-iron cylinder, 4 feet long by 15 inches in diameter, covered at the top and open at the bottom. There is a stuffing-box in its cover which allows a thin spindle to pass through it in an air-tight manner. At its lower end the spindle carries a fan which consists of a thin metallic disk 11 inches in diameter, having a hole 4 inches in diameter in its centre, and with radial blades $1\frac{1}{2}$ inches high on its upper surface. When the fan revolves, the blades, which are nearly touching the cover of the cylinder, throw out the air centrifugally and draw in new supplies through the hole in the disk. Immediately below the

hole in the disk, and concentric with it, there is a thin sheet-iron pipe 4 inches in diameter, whose upper end almost touches the disk while its lower end descends to within 4 or 5 inches of the bottom of the cylinder. When the fan is made to revolve rapidly, the air passes up through the central pipe with a velocity of 460 feet per minute. At its upper end the spindle carries a small grooved pulley, *d*, which is made to revolve by means of an endless cord passing round it and over another large grooved driving wheel D. The lower end of the cylinder rests on an iron ring screwed down to the top of the wooden gallery BC, in which there is an opening corresponding to the size of the cylinder. The cylinder is attached to one side of the iron ring by a hinge which allows it to be folded back into the position shown in fig. 4. At the other side it can be fastened by a screw in an upright position as shown in the other figures.

The gallery BC, consists of a range of wooden pipes 14 inches square inside, and, altogether, 79½ feet long. The centre of the cylinder A is 5 feet from the end B, and there is a valve just below the point *b*, by means of which the part of the gallery towards B can be isolated from the remainder. The separate pipes have broad wooden flanges which are put close together when they are placed so as to form a gallery, but they are not fastened to each other in any way; they rest on wooden blocks *a a*, and any one of them can be drawn out from between the two on each side without disturbing the others. This is shown by the dotted lines in fig. 6. Near the end B there is a sheet-iron cylinder 3 feet long by 10 inches in diameter, closed at each end, and having an inlet pipe for steam at *q*, and an outlet pipe at *t* for condensed water and steam. At the same end also there is a branch *e*, *f*, *g*, leading to K, which is the horizontal part *d c*, of the apparatus represented in fig. 1. This branch is also connected with a blowing fan F, driven by a steam turbine of which *l* and *h* are the steam and exhaust pipes. The pipe bringing fire-damp from below ground, referred to in my former paper, can be made to deliver its fire-damp into the air inlets of the fan in the same way as in the former experiments. It will now be evident that currents of air of various qualities can be made to traverse the gallery B C from B towards C. Thus, if the valve *s* is open, while the valve *s′* is shut, the return air of the upcast shaft passes through the apparatus and escapes at C; but if *s′* is open and *s* is shut, the return air is cut off, and by setting the fan F in motion, we obtain either pure air, or air and fire-damp mixed, as we may desire. In every case, also, the air passes over, and is heated by, the steam cylinder *p*, so that, even when return air is used, the interior of the gallery CD can be kept dry.

The interior of the cylinder A is lined with wood about ·2 inch thick, and its capacity is about 4·648 cubic feet. For the purpose of obtaining the explosive mixture required, the cylinder E, 8 inches in diameter,

and 2 feet long, is filled with fire-damp, of which a certain measured proportion is afterwards transferred to the interior of the cylinder A through the india-rubber tube *r n*. This is done by admitting water through an india-rubber pipe attached at the point O. At the same time as the gas is flowing in at the top of the cylinder A, air is allowed to escape at the point *b* near its bottom by taking out a plug for that purpose. The plug is put in immediately after the operation is completed. The amount of fire-damp employed is as near as may be ·456 cubic foot. The cylinder E is refilled with fire-damp by shutting a stop-cock at *n*, opening another at *m*, which is connected with the fire-damp pipe by means of an india-rubber tube not shown in the drawing, and lowering the water bucket below the level of the bottom of the cylinder E. Before the fire-damp is admitted to the interior of the cylinder A, a paper diaphragm is inserted between its lower end and the ring to which it is hinged and screwed, so as to isolate it from the gallery BC. The explosive mixture in the cylinder A is ignited by the spark of a powerful magneto-electric machine which Messrs. Cross Brothers, of Cardiff, most kindly lent to me for the purposes of these experiments. The wires pass through the plug at *b*, and are brought together just inside the cylinder.

The method of forming an explosive mixture in the cylinder A, will now be sufficiently plain, but I will repeat the description of the operations in regular order. When the cylinder is in the position shown in fig. 4, several sheets of paper are laid over the opening in the top of the gallery; the cylinder is then raised to an upright position and fastened by means of the screw. The plug *b* is opened and fire-damp is made to flow through the pipe *r n*, displacing a corresponding volume of air which escapes at *b*. As soon as the requisite volume of gas has been obtained the cock *r* is shut and the plug *b* is replaced. The driving wheel D is next made to revolve at the rate of about eighty turns per minute; twenty-five or thirty turns being found quite sufficient to make a perfect mixture; and, thereafter, a spark from the magneto-electric machine causes the explosion.

When there is no coal-dust in the gallery BC, the flame of the fire-damp explosion does not extend further than from 7 to 9 feet from the bottom of the cylinder A. It should be understood that the valve at *b* is always closed just before the spark is passed.

When the gallery contains coal-dust, on the other hand, scattered along its floor, and lying on a few shelves, whose position will be given immediately, and when it is filled with the return air of the upcast shaft, the flame of the explosion traverses its whole length, and shoots out into the air at the end of C, to distances varying from from 4 to 15 feet beyond it. At first, it appeared to me that the wooden gallery might be prolonged indefinitely with the same result; but on adding another pipe at the end of C, I was surprised to find

that I could not, by any possibility, get the flame to travel more than one-half or two-thirds of the former distance, and I came to the conclusion that the initial impulse, which raises the coal-dust, is insufficient to overcome the resistance under the altered conditions. Again, I had 60 feet of nearly air-tight pipe prepared, thinking thereby to prevent the energy of the wave created by the fire-damp explosion from being dissipated; but here, once more, I found that it was impossible to get the flame to travel to a distance of more than 30 or 40 feet from the origin, and in this case I concluded that the expanded part of the wave extinguished the flame of the coal-dust. The best results were obtained when the wooden pipes had open seams along the junction of the boards of which they are formed.

At the beginning of the present month (March, 1879), Professor G. G. Stokes, F.R.S., communicated to me the suggestion that if a weak solution of chloride of calcium were used for watering the roadways of mines, instead of ordinary water, the deliquescent salt would tend to retard evaporation, and a smaller quantity of water would serve the purpose of keeping the workings damp. Accordingly, I have begun an experiment with such a solution in a dry mine, but it is not yet sufficiently advanced to enable me to state any results in the present paper.

The temperature of the air current passing along the gallery varied from 74° F. near the explosion-cylinder to 60° at the end C. The wooden shelves spoken of above were in sets of three (one above the other at equal distances) the shelves themselves being about 6 inches broad. One set was placed at each of the points ×, fig. 6; and a brick was placed so as to obstruct the passage below the lowest shelf of the first, third, and fourth sets for the purpose of causing the force of the explosion to exert itself more powerfully in sweeping the dust off the shelves and mixing it with the air.

The arrangements whereby pure air, or pure air and fire-damp, can be employed, were only completed before the weather became unsuitable for continuing the experiments, which, I need hardly say, are made in the open air. I obtained sufficient results, however, to show that the absence of even the small proportion of fire-damp contained in the return air of Llwynypia Colliery, makes a great difference in the force of the explosion and the distance to which the flame will travel along the gallery. I found, also, that when two per cent. of fire-damp was added to the current of pure air entering the fan F, even better results were obtained than with the return air of the mine.

Although this apparatus appears to be on too small a scale to solve the coal-dust question unequivocally, I think the results obtained with it are sufficiently conclusive to enable us to affirm, that a fire-damp explosion, occurring in a dry coal mine, is liable to be in-

definitely extended by the mixture of air and coal-dust produced by the disturbance which it initiates.

The dangers due to the presence of coal-dust in dry mines can be very easily avoided by sprinkling water plentifully on the principal roadways along which the air currents pass, in going to, and coming from, the working places. For example, Llwynypia Colliery, which was formerly one of the driest and most dusty of the mines in the South Wales basin, is now kept constantly damp or wet in this way with a daily expenditure of about 1,800 gallons of water. The amount of air passing through it at present is over 80,000 cubic feet per minute, and its out-put of coal is, on the average, about 800 tons per day.

III. "The Contact Theory of Voltaic Action." No. III. By Professors W. E. AYRTON and JOHN PERRY. Communicated by Dr. C. W. SIEMENS, F.R.S. Received February 19, 1879.

(Abstract.)

The authors commence by referring to the experiments that had been made prior to 1876, on the difference of potentials of a solid in contact with a liquid, and of two liquids in contact with one another, and they point out that:—

1. The earlier experiments were not carried out with apparatus susceptible of giving accurate results.

2. Owing to the incompleteness of the apparatus assumptions had to be made not justified by the experiments.

3. *No direct* experiments had been performed to determine the difference of potential of two liquids in contact, with the exception of a few by Kohlrausch, using a method which appeared to the authors quite inadmissible as regards accuracy of result.

In consequence of this great vagueness existed as to whether the contact difference of potentials between two substances, when one or both were liquids, was a constant depending only on the substances and the temperature, or whether it was a variable dependent upon what other substance was in contact with either. Some authorities regarded it as a variable. Gerland considered he had proved it to be a constant, but first, the agreement of the value of the electromotive force of each of his cells with the algebraical sum of the separate differences of potential at the various surfaces of separation, and which was the test of the accuracy of his theory, was so striking, and so much greater than polarisation, &c., usually allows one to obtain in experiments of such delicacy, that one could not help feeling doubtful regarding his

conclusions; secondly, his apparatus did not allow of his experimenting with two liquids in contact, consequently he could not legitimately draw any conclusion in this latter case. And although Kohlrausch had made some few experiments on the difference of potentials of liquids in contact, still since he employed moist blotting paper surfaces instead of the surfaces of the liquids themselves, the authors considered for that reason alone, if for no other, that his results did not carry the conviction the distinguished position of the experimenter might have led them to anticipate.

They therefore designed a method and an apparatus for carrying it out, by means of which they could measure the difference of potentials in volts at each separate contact of dissimilar substances in the ordinary galvanic cells, from which they could ascertain whether the algebraical sum of all the contact differences of potential was, or was not, equal to the electromotive force of the particular cell in question. From the results they obtained, and which are given in Papers Nos. I and II, "Proc. Roy. Soc.," No. 186, 1878, they concluded within the limits of their experiments that if $\overline{AB}$, $\overline{BC}$, $\overline{CD}$, &c., were the contact differences of potential measured separately of the substances A in contact with B, B in contact with C, &c., then, any one or more of the substances being solid or *liquid*, if any number $A, B, C - - - - - K$ were joined together, and the electromotive force of the combination $\overline{AK}$, measured, the following equation was found true:—

$$\overline{AK}=\overline{AB}+\overline{BC}+\overline{CD}+ - - - - - +\overline{JK},$$

which proved that each surface of separation produced its effect independently of any other.

Their method by which any single contact difference of potentials was measured was as follows:—Let 3 and 4 be two insulated gilt brass plates connected with the electrodes of a delicate quadrant electrometer. Let 1 under 3, and 2 under 4 be the surfaces whose contact difference of potential is to be measured; 3 and 4 are first connected together and then insulated, but remain connected with their respective electrometer quadrants. Now 1 and 2 are made to change places with one another, 1 being now under 4 and 2 under 3, then the deflection of the electrometer needle will give a measure of the difference of potentials between 1 and 2; and in the present paper it is proved that in order that the observed difference of potentials in the electrometer quadrants shall be proportional to the contact difference of potentials desired to be measured, either there must be perfect symmetry in the induction apparatus before and after reversal of 1 and 2, a condition very difficult to be obtained, or else the plates 3 and 4 must, in addition to being connected together, be also put to earth, or reduced to zero potential, before each reversal, and also the mean potential of the substance under test

must be kept as low as possible, conditions that were always carefully observed in the experiments.

The apparatus employed by the authors in the present investigation is then explained in detail, and it is shown how, by improving on their earlier form, they have removed a difficulty which formerly existed, and which prevented their previously experimenting on pairs of substances having very different weights, such as a vessel of mercury and a sheet of metal. The method of making the permanent and temporary adjustments, the tests for leakage, the experimental mode adopted of compensating the error arising from defects in parallelism of the apparatus affecting the results obtained from two rigid surfaces (as that of copper and zinc) differently from the result found with one or with two liquid surfaces under test are then described, as well as the details of a complete experiment and the precautions adopted to obtain clean metallic surfaces.

The authors explain that the results they have obtained in this investigation have divided themselves into three groups:—

1st. The contact difference of potentials of metals and liquids at the same temperature.

2nd. The contact difference of potentials of metals and liquids when one of the substances is at a different temperature from the other in contact with it, for example, mercury at 20° C. in contact with mercury at 40° C.

3rd. The contact difference of potentials of carbon and platinum with water, and with weak and with strong sulphuric acid;

but that they give only the results under head No. 1 in the present communication, reserving those they have obtained under heads Nos. 2 and 3 for a future occasion.

Then follow arranged in the order in which they were obtained from January to May, 1878, some 150 results of experiments (each number being on the average the mean of eight observations), representing the contact differences of potential of nine solids and twenty-one liquids. They explain that the numbers given are those to which alone they attach importance; but that in consequence of much time having in such delicate experiments to be spent in obtaining measurements, which are often found out to be wrong, a considerable number of results have been rejected, and are not mentioned in the paper, and that this, therefore, explains why in some cases one measurement only is apparently the result of a whole day's work. These remarks especially apply to the authors' attempts to measure the contact difference of potentials between a liquid and a paste, for example, mercury and mercurous sulphate paste; great difficulty being introduced by the extremely thin layer of water on the surface of the paste acting inductively instead of the paste itself. They mention that this difficulty is a very good example of the inaccuracies that

must have been introduced by former experimenters using a moist blotting-paper surface instead of the surface of the liquid itself.

A large number of discordant results were obtained in March, 1878, and their explanation led to the interesting result that the apparent contact difference of potentials between a metal and mercury, as measured inductively, varied much with small additions of temperature. The accidental difference of temperature in the different experiments arose from the mercury having been redistilled in the laboratory between every two experiments to remove all possible traces of impurities, and probably in some cases it had not become perfectly cold before a new experiment was made. The investigation of this apparent change of contact difference of potentials with temperature led to a consideration of the contact difference of temperature of mercury with air, since, of course, in all these inductive experiments two air contacts are included in the result. The results thus obtained will form part of the substance of the next paper.

Next follow a number of checks of the accuracy of the results based on the well known law that in any compound metallic circuit at uniform temperature there is no electromotive force. This is followed by some considerations regarding the measurement of the difference of potentials between substances and the air in contact with them, and of measurements of the Peltier effect.

It has usually been thought that the differences of potential of liquids in contact with one another were so small as to be almost inappreciable in comparison with the differences of potential of metals in contact; but the authors have ascertained, among other results, that strong sulphuric acid in contact with distilled water, solutions of alum, copper sulphate, and zinc sulphate has a measured difference of potentials of 1·3 to 1·7 volts, an electromotive force more than twice as high as that of zinc and copper in contact. And hence the importance of an apparatus that can directly measure the difference of potentials of two liquids.

March 20, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read :—

I. "Note on some Spectral Phenomena observed in the Arc produced by a Siemens' Machine." By J. NORMAN LOCKYER, F.R.S. Received March 3, 1879.

In continuation of my work on the spectra of metallic vapours photographed when incandescent in the electric arc, I have recently employed a Siemens' dynamo-electric machine driven by a gas engine of ten horse-power, using an effective force of five or six.

The greatly increased length of arc obtained by these means has enabled me to observe and photograph a new set of phenomena of great beauty, and I think of the highest theoretical importance.

In my former work with a battery of thirty cells, in order to obtain the lengths of the lines, it was necessary, in consequence of the shortness of the arc, to throw an image of a horizontal arc on the vertical slit of the spectroscope. In this manner perfectly symmetrical photographs were obtained, the shortest lines due to the core, and the middle portions of the longest ones proceeding both from the core and the exterior portion, lying in the axis of the photograph.

With the Siemens' machine the arc is not only much longer, but when some substances are introduced into it, it is accompanied by a flame sometimes three or four inches long, of great complexity both with regard to colour and concentric envelopes.

The spectrum of this flame was first photographed side by side with that of the arc itself, and when the poles are clean the flame has been shown by eye observation to be chiefly due to the oxidation of the carbon and calcium vapours which exist in the free state in the air, thus giving us absolute demonstration of a combination brought about among vapours by reduction of temperature.

These flame phenomena also give us an opportunity of observing the inverse appearance of spectral lines, to which my attention has lately been much drawn. The following is a case in point:—In one photograph of the flame given by manganese the line at wave-length 4234·5 occurs without the triplet near wave-length 4030, while in another photograph the triplet is present without the line at 4234·5.

The various phenomena presented in these photographs, especially the greater breadth of the reversals in the case of some of the metallic lines in the flame, and the gradual introduction of new spectra, led me to imagine that in different regions *of the arc itself* the spectroscopic effects might vary greatly; and to test this I very carefully projected the image of a vertical arc on the slit, focussing that particular light which I was about to photograph.

In the plates thus obtained the spectra of those portions of the arc adjacent to the positive and negative poles are widely dissimilar.

We may say, roughly, that the carbons employed give a spectrum containing the flutings of carbon and the lines of calcium, some of them reversed, as I have shown many years ago, both in the case of this and other substances.

In the photographs of the arc taken under the conditions I have stated, the calcium lines cling to one pole and the carbon flutings to the other; the lines which reverse themselves in the case of calcium being those which have their intensity most pronounced close to the pole. That this is a phenomenon not dependent upon the chemical constitution of the two poles, but rather on some electrical separation, is rendered evident by the fact that on changing the direction of the current the calcium and carbon spectra change positions.

Although these phenomena are very marked in the photographic region, as is evidenced by the photographs which I submit to the Society, they yet appear more strongly developed in the less refrangible regions. The exquisite carbon flutings in the yellow-green, for instance, cling more closely to the pole than those in the violet.

So much for the spectrum of the poles themselves.

If now we introduce a metal and observe its vapour, we find a perfectly new set of phenomena. We get long and short lines, but the law which they obey is no longer the one in operation when the parts of the arc examined are symmetrical with reference to the positive and negative poles as when a horizontal arc is employed.

Some lines stretch across the spectrum with their intensities greatest close to one pole, while other lines invisible at this pole are most intense at the other. *In one photograph, for instance, the blue line of calcium is visible alone at one pole, the H and K lines without the blue lines at the other.*

More than this, there is a progression of lines, so to speak, from pole to pole. They lie *en échelon* along the spectrum.

In the case of other lines, only the central region is occupied, the line being enormously distended either like a spindle or a half-spindle, with the bulging portion in some cases on the more, in others on the less, refrangible side.

It is very difficult to understand what process is here at work if we are not in presence of separations brought about by temperature and electricity.

However this may be, a most convenient method is afforded of separating basic from non-basic lines. I have already, in previous communications, referred to the repetition of doublets in some spectra, and of triplets in others; and it often happens that one member of a doublet or triple group is basic. The wide iron triplet near G, to which I referred in my communication read on the 12th December, has its central member basic with calcium; and this is most beautifully shown by the extension of the iron triplet right across the arc, while

the central member alone is thickened along with the other calcium lines on one of the poles.

LIST OF PHOTOGRAPHS EXHIBITED.

Photograph of Mn lengths taken in the old way, showing that with a battery of 30 Grove cells and a horizontal arc a perfectly symmetrical photograph was obtained with the lines extending equally on either side of a horizontal ink line drawn through the axis of the photograph.

Photographs of Ba, Sr, and Mn, giving in each case the spectrum of the core compared with that of the flame of the arc.

I. Ba, showing that of two lines (wave-length 4130·5 and 4282·5) of equal intensity in the core, the less refrangible was visible almost alone in the flame.

II. Sr, showing two lines at 4078·5 and 4215·3. In the core the more refrangible has a much broader reversal than the other, while in the flame the less refrangible exists alone and reversed.

III and IV. Mn, in the flame-spectrum of one of these photographs the triplet at about 4030·0 exists with no other Mn lines; while it is absent from the flame-spectrum of the other photograph, although the Mn line at 4234·8 is present.

Photograph of flame-spectrum of Ca taken with an oblique slit, as the flame nearly always branches off at an angle.

Spectrum of Rb, showing that the carbon bands cling to the hotter pole, while the Ca and Rb lines cling to the opposite pole; one set of carbon bands, however, stretches right across the spectrum.

Two photographs showing reversal of phenomena by reversing the current.

I. Two spectra of Pb, obtained by normal current and reversed current, showing that the lines which, in the upper spectrum were thickened at their lower extremities, were in the lower spectrum thickened at their upper extremities, and that the general appearance of the two sets of lines was reversed.

II. Two spectra of Cu showing the same phenomena.

Spectrum of Mn obtained with the large arc of a Siemens' machine, in which the want of symmetry is so conspicuous that if a straight line be drawn through the centre of the photograph it will cut one set of lines at their centres, another near their upper extremities, while a third set of lines will be cut near their lower extremities.

Photographs showing separation of lines.

I. Spectrum of Li containing impurity lines of Ca, Sr, Fe, and Mn, the Ca and Mn lines clinging to one pole, and the Fe and Sr lines to the other.

II and III. Different parts of Fe spectrum, showing Ca lines starting from one pole, and Fe lines from the other.

IV. Spectrum of Cu, showing that this separation can exist not only between two metals, but even between lines of the same metal. In this the blue Ca line is seen thickened at the upper pole, while the H and K lines were only seen at the lower pole.

Spectra of Ti, Ni, and Mn, in which the lines were not all produced at the two poles, some occupying an intermediate position; the lines thus arranging themselves *en échelon* along the spectrum.

Spectra of Cu and Ni, showing the irregular thickening of different lines at different levels of the arc.

Photographs of the spectra of Sr, Cu, and Mg, showing central thickening.

I. Sr, containing two lines, one Ba showing very little thickening, while the other true Sr line exists only at the centre as a broad, fluffy reversed line.

II. Cu, all the Cu lines in this photograph have their central portions expanded, and most of them to a greater extent on the less than on the more refrangible side. Some of the Fe lines have also their centres expanded, which is not the case with the Ca lines, the Ca lines in the blue being developed at their extremities.

III. Mg, showing line in the blue-green, with a central expansion on the less refrangible side, giving the line the appearance of a half spindle, *b* being quite normal.

Spectrum of Sn; showing lines of Ca and Fe; the former are expanded at their lower extremities, the Fe lines are not so expanded, but exist at a higher level than the Ca lines. The photograph shows that the line which is basic to Fe and Ca is carried up with the other Fe lines, and is also expanded at its lower extremity with the other Ca lines.

II. "Note on some Phenomena attending the Reversal of Lines." By J. NORMAN LOCKYER, F.R.S. Received March 5, 1879.

In the "Phil. Trans." for 1873, page 253, I gave an account of an experiment devised by Dr. Frankland and myself, in which the absorption line of sodium was made to vary considerably in thickness, owing to the variation in the quantity of sodium vapour which was produced in a tube when a mass of metallic sodium was heated in an atmosphere of hydrogen.

In the "Phil. Trans." for 1874, vol. clxiv, Part II, p. 805, speaking of the photographs of arc spectra which I had then commenced to take, I stated, "it not unfrequently happens that a very thick line will reverse itself, a circumstance which greatly facilitates its comparison with confronted lines, since a thin dark line then runs down the centre of the thicker bright one," and I pointed out in a note that the absorption line does not always occupy the exact centre of the bright band. I gave examples of this from the spectra of calcium and aluminium, the examples being reproductions of photographs of the arc. These were published with the paper.

In other subsequent communications, I have referred to these reversals, and I have elsewhere made general statements with regard to them, and drawn attention to the distinction between those substances which give us winged lines in arc spectra and those which do not.

If the method of throwing an image of the arc upon the slit be employed, a method which I suggested and utilised in 1870,* for

* "Phil. Trans.," 1873, p. 254.

terrestrial substances, there is no difficulty in seeing the reversal of winged lines in the case of all spectra in which they exist, and such lines lying in the region between K and G have been photographed and exhibited to the Society on several occasions in connexion with one part or another of my researches. When a lamp of thirty cells, however, is used, although the various curious phenomena which these reversals present are easily visible, it is very difficult to photograph them.

The longer arc given us by the Siemens' machine to which I have referred in another communication has enabled me, however, to photograph several of the various aspects put on during the process of reversal; these photographs I exhibit to the Society; chief among these phenomena are the various thicknesses of the lines of reversal over the arc and poles, and the appearance of the bright line without reversal in some regions, and the reversal without bright line in others.

All the phenomena presented by the absorption of the D line to the eye are here in duplicate. It may be useful, perhaps, to state what phenomena are seen in the case of the D line, when a small image of the arc, carefully focussed for the yellow light, is thrown upon the slit and considerable dispersion is employed.

If the arc is observed before the introduction of the sodium on to the poles, with the poles slightly separated, the continuous spectrum of each pole will be bounded by a sharp line, and in the included region the exquisite flutings of the carbon vapour will be seen together with the lines due to any metallic substances present. The metallic lines will be thickest near one pole, and will overlap its continuous spectrum, while the carbon flutings will overlap the other. The D lines in the arc should occupy the centre of the field of view.

If now a piece of metallic sodium be placed on the lower pole, the whole of the light will be blotted out, if the field of view be small. Gradually the two ends of the spectrum of the arc will begin to appear on either side of the field, the sharp boundary lines to which reference has been made having disappeared, as the poles are no longer incandescent.

The absorption in its retreat to the central region will next take the appearance of a truncated cone, its base resting on that side of the arc formerly occupied by the carbon flutings. The intense blackness gradually changes into a misty veil through which, as it were, the D lines gradually make their appearance as enormous truncated cones with their bases turned in the opposite direction to that occupied by the original absorption.

The more refrangible line is twice as thick as the other, and is often contorted while the other is rigid. Gradually, as the quantity of sodium vapour is reduced, the poles regain their original incandescence,

and the one to which the carbon bands attach themselves will become more vividly incandescent than the other. Then begins a new set of phenomena—the absorption of the light of either pole. Generally on the more incandescent pole the absorption widens for a space, then narrows and finally puts on a trumpet appearance and is lost, as if the molecules to which the absorption is due were then, owing to the reduction of temperature, being reconstructed, thus increasing the quantity of available absorbing material of this particular kind.

Very often on the opposite pole the line is seen merely as a bright one, or again the absorption is reduced to its smallest proportions.

Having thus stated the phenomena with regard to the D line, it will be convenient to make some general statements supported by the various photographs which I now submit to the notice of the Society.

I. We have first a general absorption of the light of the arc over the region to be eventually occupied by the bright line.

II. Next the disappearance of this indefinite absorption and the formation of a truncated absorption of a symmetrical bright and wider line.

III. Next the parallelism of the boundaries of the bright and dark lines in the centre of the arc itself.

IV. Next the various absorption phenomena on the two poles.

V. Finally the extinction of the absorption line in the arc.

The other lines in the sodium spectrum are also good representatives of cases in which the absorption leads to different appearances, or in which absorption phenomena are entirely wanting. For instance, the double green line of sodium shows scarcely any trace of absorption when the lines are visible; but before the lines are produced out of a general brightness which fills the whole field the absorption is visible as line absorption, the less refrangible member being thicker and darker than the other, exactly the opposite to what holds with the D lines.

I have observed no absorption in the case of the blue line, but the radiation phenomena are extremely curious taken in connexion with the other lines. While the D lines put on the appearance of black truncated cones, while the green lines widen at their bases towards the red and not at all towards the more refrangible side, the blue lines are only widely developed in the centre of the arc, and are least developed in that portion of it where the phenomena of the other lines are seen in their strongest intensity; thereby affording a striking instance of the irregular absorption and radiation of the molecules of the same element in the same sectional plane of the arc.

The red double line of sodium I have never seen reversed nor irregularly widened.

When a Siemens' lamp is employed, the absorption phenomena of *the flame referred* to in another communication merit a most careful

study. The lines which reverse themselves most readily in the arc are generally those, the absorption of which is most developed in the flame; thus the manganese triplet in the violet is magnificently reversed in the flame, and the blue calcium line is thus often seen widened, H and K being not only not absorbed, but entirely invisible.

LIST OF PHOTOGRAPHS EXHIBITED.

Photographs showing passage from truncation to parallelism.

I. Spectrum of Sr, showing two reversed lines (wave-lengths 4078·5 and 4215·3) gradually broadening towards one end.

II. Spectrum of Ca, showing reversal of the blue line, and of H and K. While the blue line presents the appearance of a cone, through the centre of which is the absorption line bounded by parallel sides, the H and K lines are almost normal in their appearance, showing, however, a slight widening at one extremity.

III. Spectrum of Mn, showing blue Ca line tapering to a point at one extremity and enlarging spindle-shape towards the other end; the reversal of this line does not extend through its whole length, but merely through the bulging portion, tapering gradually to a point.

IV. Spectrum of Sr, showing the two lines (4078·5 and 4215·3) which this time present an appearance very similar to the blue Ca line in the last photograph. In the more refrangible line, however, the reversal retaining its tapering form extends through the whole length of the line.

Two photographs of the spectrum of Ca, in which not only the blue line but also the H and K lines present the appearance of truncated cones.

Spectrum of Mn, showing the absorption of its triplet (at wave-length about 4030) without its radiation.

Spectrum of Mn, in which the triplet is again reversed. Here the triplet, together with its two included bright lines, looks exactly like a group of eight radiation lines, each reversed line giving the appearance of two bright lines.

Photographs showing non-symmetrical lines.

I. Spectrum showing two Ag lines at about wave-lengths 4054·3 and 4210·0. Both lines are fluffy and reversed; the less refrangible line is much more strongly expanded on its more refrangible side, and is carried up to a much greater height as a radiation line than its other side. The more refrangible line is more symmetrical, but presents the same phenomena to some extent, only in the opposite direction, its less refrangible side being the most developed.

II. Spectrum of Rb, showing line at wave-length 4202. Here the two ends of the line are produced by radiation alone, the central portion showing absorption on its more refrangible side with fluffy shading on its less refrangible side.

Spectra of Sr and Cs, showing the absorption of light due to the poles.

Photographs showing the trumpeting of lines.

I. Spectrum of Ca, in which the reversal is seen to widen as we approach the faint end produced by the cooler external region of the arc, thus showing absorption increasing with reduction of temperature.

II. Another spectrum of Ca, showing the same thing again.

III. Spectrum of Pb, showing that the Pb line at wave-length 4058 also trumpets.

IV. Spectrum showing the Ba line at 4553·4 trumpeting. Here the line, after proceeding to a considerable distance from the hottest region of the arc as a fine reversed line, gradually expands towards its extremity.
Flame-spectrum of Mn, showing the reversal of the triplet in the arc-flame.
Flame-spectra of Ca, showing the gradual extinction first of K and then of H as the flame recedes farthest from the arc.

III. Discussion of "Young's List of Chromospheric Lines." (Note I.) By J. NORMAN LOCKYER, F.R.S. Received March 5, 1879.

[PLATE 9.]

In my paper read on the 12th December, 1878, I called attention to the fact that, in the case of the metals discussed in that paper, with the exception of hydrogen, there was a considerable discrepancy between the intensities of the lines seen in our laboratories and the number of times the lines had been seen by Young in his careful researches on the chromosphere.

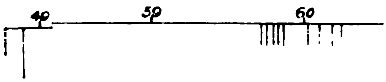
In a preliminary note " On the Substances which produce the Chromospheric Lines " I pointed out that the lines visible in the spectrum of the chromosphere when a metallic prominence is observed are for the most part basic lines, that is to say, with few exceptions, the longest and brightest lines visible in the spectra of the so-called elements are conspicuously absent; instead of them we find fainter lines, which Thalén has, in many instances, mapped as common to two elements.

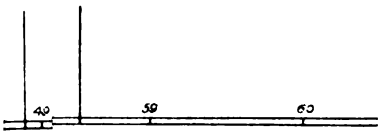
Since these papers were communicated to the Society I have continued this line of inquiry, and I now propose to state what I have thus far done:—

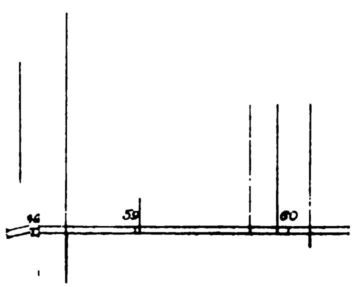
1. The maps of the spectra of calcium, barium, iron, and manganese, submitted to the Society in an incomplete state when the preliminary note was read, have been completed. In these the lengths of the lines in the spectra of the metallic elements represent the intensities given by Thalén, whose lines and wave-lengths I have followed in all cases, while those of the lines visible in solar storms, represent the number of times each line has been seen in the spectrum of the chromosphere by Professor Young, to whose important work I have drawn special attention in my last two communications. An inspection of these maps is sufficient to show that there is no connexion whatever beyond that of wave-length between the spectra; it will be gathered from the maps how the long lines seen in our laboratories are suppressed and the feeble lines exalted in the spectrum of the chromosphere, see Plate 9. The Mn map has been omitted on account of its excessive complication.

2. I have discussed the coincidences recorded in Ångström's map and Thalén tables in the sheets of the " Spectre Normal," comprising

[redacted]







the wave-lengths 4120—5400. I have discussed in each case the possibility or impossibility of such coincidence having arisen from the presence of an impurity. Any person going over this table after he has taken the trouble to acquire the information necessary to understand it, will see that in a large number of cases the coincidences cannot be ascribed to the existence of impurities. In many cases, it is true, the coincidences *may* arise from the presence of impurities, but this is by no means a proof that they do so. Indeed it does not appear to have struck all who have considered this question, that, as I have before shown, the presence of B existing with A as an impurity and in A as a base, will, up to a certain point, give the same results; the ascribing of lines, therefore, to impurities, without a demonstration of the impurity, is an unscientific proceeding.

In this table, as in the maps, it will be seen how the faintest lines are apt to be most frequently seen in the chromosphere. (See pp. 434—439.)

3. I have attacked Young's complete list from another point of view, discussing, in connexion with Ångström's map and Thalén's tables, all lines seen less than one hundred and more than fourteen times, to determine whether, when treated in this way, there was any connexion between the intensities of the lines, as given by Thalén, and the number of reversals, including, of course, those cases in which Thalén has assigned the line to two metals.

Of the forty-one lines given in the following table, no less than five have exactly the same readings in two metals according to Thalén, and three more have very small differences. It will be observed that only one line of the 1st order of intensity in the spectrum of iron appears in the list. This was observed three times, while two lines of the 3rd order have been seen no less than forty times. No line of manganese above the 3rd order has been observed, and of the two recorded, a 5th order line has been observed twenty times, and a 3rd order line fifteen.

It will be seen, further, from the last column in the table, that, as a rule, when we leave out of discussion the lines visible in Sirius, the more intense adjacent lines of the same metals have either not been seen at all by Young, or have been seen less frequently. (See pp. 440, 441.)

4. I have made some preliminary observations on the presence in, or absence from, metallic spectra of some of the lines most frequently seen by Young, for if the lines observed so frequently by Young in solar storms and recorded as common to two substances at least by Thalén be really basic, it becomes highly probable that these lines are really present in the spectra of many bodies but have been overlooked by previous observers in consequence of their faintness.

Up to the present time my work has been somewhat restricted in this

Discussion of Metallic Coincidences in Thalén's Map.

Sheet containing E and F.

Wave-length of line and number of times seen by Young.	Intensity in Sun.	Metallic coincidences, Thalén, and Intensities.	Remarks.
5352·4 Y 4	3	5352·4 Fe (4) 5352·4 Co (3)	This is not likely to be due to an impurity of Co in Fe, as the nearest Co line (5351·2) of equal intensity is not in the Fe spectrum; neither can it be due to an impurity of Fe in Co, as the second intensity Fe line (5340·2) is absent from the Co spectrum. This, therefore, cannot arise from impurity.
5348·6	3	5348·6 Fe (4) 5348·6 Ca (2)	The second intensity Fe line 5340·2 being absent from the Ca spectrum, this cannot come from an impurity of Fe in Ca. There is not sufficient evidence in this region of the map to show that this is not due to an impurity of Ca in Fe.
5269·5 Y 15	2	5269·5 Fe (1) 5269·4 Ca (2)	
5265·8 Y 10	2	5265·8 Fe (2) 5265·8 Co (3)	This line being marked considerably more intense in Fe than in Co, cannot be due to an impurity of Co in Fe; neither can it be due to an impurity of Fe in Co, as the first order Fe line at 5268·5 is absent from the Co spectrum. This line, therefore, cannot arise from impurity.
5254·1 Y 1	4	5254·1 Fe 5254·1 Mn (4)	This line is omitted from Thalén's Fe table, and is marked as a very weak line in the map; it, therefore, cannot be due to an impurity of Fe in Mn; it is not likely to be an impurity of Mn in Fe, being only a fourth intensity Mn line, the long Mn lines in other parts of the spectrum being absent from the Fe spectrum. This line is, therefore, not due to an impurity.
5234·4	5	5234·4 Fe 5234·4 Co (5)	Fe line, faint in map, is omitted from Thalén's table. Fe line of first intensity 5232·1 is not in Co spectrum; this, therefore, cannot be due to an impurity of Fe in Co. Neither can it be due to an impurity of Co in Fe, being one of the faintest Co lines, and the long Co lines in other parts of the spectrum being absent from the Fe spectrum. This line, therefore, cannot arise from impurity.

Wave-length of line and number of times seen by Young.	Intensity in Sun.	Metallic coincidences, Thalén, and Intensities.	Remarks.
5207·6 Y 10	2	5207·6 Fe (3) 5207·6 Cr (1)	This is clearly not due to an impurity of Fe in Cr, being so much stronger in the latter metal, while the first intensity Fe lines in the same field are absent from Cr. It cannot be due to an impurity of Cr in Fe, as the Cr line of equal intensity at 5205·2 has no coincidence in the Fe spectrum. This, therefore, cannot arise from impurity.
5203·7 Y 10	2	5203·7 Fe (3) 5203·7 Cr (1)	This coincidence cannot arise from impurity for the same reasons as last.
5168·3 Y 40	1	5168·3 Fe (3) 5168·3 Ni (5)	This line cannot be due to an impurity of Fe in Ni, as the second order Fe line at 5138·6 is absent from Ni. There is not sufficient evidence in map to show that it is not due to an impurity of Ni in Fe.
5166·7 Y 30	1	5166·7 Fe (2) 5166·7 Mg (1)	This line cannot be due to an impurity of Fe in Mg, as the second order Fe line at 5138·6 is absent from the Mg spectrum. Neither can it be due to an impurity of Mg in Fe, as b_1 and b_2 are absent from the spectrum of Fe.
5145·7	5	5145·7 Fe 5145·7 Ni (5)	Fe line omitted from Thalén's table; coincidence marked in map. Cannot be due to an impurity of Fe in Ni, for the second intensity Fe line at 5138·6 is not in Ni, this line being faint in Fe. There is not sufficient evidence in this portion of the spectrum to prove that it cannot be due to an impurity of Ni in Fe; but since in other parts of the spectrum the long Ni lines are not in Fe, it is impossible for a fifth intensity line to be present. This line, therefore, cannot arise from impurity.
5142·0	5	5142·0 Fe 5142·0 Ni (5)	" " " "
5136·8	3	5136·8 Fe 5136·8 Ni (5)	" " " "
5041·2 Y 2	3	5041·2 Fe (3) 5041·2 Ca (2)	This cannot be due to an impurity of Fe in Ca as the second intensity Fe line at 5049·4 is not present in the Ca spectrum. There is not sufficient evidence to show that this line is not due to an impurity of Ca in Fe.

Wave-length of line and number of times seen by Young.	Intensity in Sun.	Metallic coincidences, Thalén, and Intensities.	Remarks.
5019·4	4	5019·4 Fe 5019·4 Ti (2)	Fe line omitted from Thalén's tables; coincidence marked in map, and the Fe line being faint cannot exist in Ti as an impurity, as the second intensity Fe line at 5049·4 is absent from the Ti spectrum. Neither can it be due to an impurity of Ti in Fe, as the first intensity Ti line at 5013·3 is absent from the Fe spectrum. This, therefore, cannot arise from impurity.
5006·6	3	5006·6 Fe 5006·6 Ti (1)	Fe line omitted from Thalén's tables; coincidence marked in map. This line being faint in Fe and of intensity 1 in Ti, it cannot be shown that it may not be due to an impurity of Ti in Fe.
4990·3	3	4990·3 Fe (4) 4990·3 Ti (1)	This cannot be due to an impurity of Fe in Ti, the first intensity Fe line 4956·7 being absent from Ti. There is not evidence enough to show that it is not due to an impurity of Ti in Fe.
4983·3	3	4983·3 Fe 4983·3 Ni (5)	Faint Fe line, omitted from Thalén's tables; coincidence shown in map cannot be due to impurity of Fe in Ni, as the first order Fe line at 4956·7 is absent from Ni; neither can it be due to Ni in Fe, as the third intensity Ni line at 5016·5 is not shown in the Fe spectrum. This, therefore, cannot arise from impurity.
4884·5	3	4884·5 Fe 4884·5 Ti (1)	Fe line omitted from Thalén's tables; coincidence marked in map. This line being faint in Fe and of intensity 1 in Ti, it cannot be shown that it may not be due to an impurity of Ti in Fe.
4877·4	2	4877·4 Fe (3) 4877·4 Ca (3)	This cannot be due to an impurity of Fe in Ca, as the first intensity Fe line at 4890·4 is not present in Ca. There are no strong Ca lines in this region, so that it cannot be shown that this may not arise from an impurity of Ca in Fe.
4839·0	5	4839·0 Fe 4839·0 Co (1)	Faint Fe line, omitted from Thalén's tables; coincidence shown in map. Cannot be impurity of Fe in Co, as it is too faint in Fe. No evidence to show that it is not due to Co in Fe.
4785·8	5	4785·8 Fe (5) 4785·8 Ni (2)	This cannot be due to an impurity of Fe in Ni, as the fourth order Fe line at 4859·2 is not in Ni; neither can it be due to an impurity of Ni in Fe, as the first order Ni line of 4713·7 is absent from the Fe spectrum. This, therefore, cannot arise from impurity.

Discussion of Metallic Coincidences in Thalén's Map.

Sheet containing G.

Wave-length of line and number of times seen by Young.	Intensity in Sun.	Metallic coincidences, Thalén, and Intensities.	Remarks.
4709 ·4	3	4709 ·4 Fe (5) 4709 ·0 Ti (2)	
4690 ·8	3	4690 ·8 Fe (3) 4690 ·6 Ti (2)	
4653 ·9	2	4653 ·9 Cr (4) 4653 ·4 Fe (3)	
4646 ·4	2	4646 ·4 Fe 4646 ·4 Cr (4)	This Fe line is omitted from Thalén's table, but is marked as a coincidence in map. This cannot be due to an impurity of Fe in Cr, as there is a third intensity Fe line at 4632·0 not in the Cr spectrum. There is not sufficient evidence to prove that it is not due to an impurity of Cr in Fe.
4585 ·3	3	4585 ·3 Fe 4585 ·3 Ca (4)	Fe line omitted from Thalén's tables, but is marked as a coincidence in map. Fe line of third intensity at 4591·9 not in Ca, therefore it cannot be an impurity of Fe in Ca. Ca line of first intensity at 4454 not in Fe. This, therefore, does not arise from impurity.
4580 ·8	2	4580 ·8 Fe 4580 ·8 Ca (4)	Same reasoning applies.
4578 ·3	4	4578 ·3 Fe 4578 ·3 Ca (4)	" "
4524 ·4 Y 3	3	4524 ·4 Fe 4524 ·4 Ba (3)	This cannot be due to an impurity of Fe in Ba, for the reason that the last is not due to Fe in Ca, and it cannot be due to an impurity of Ba in Fe, for the first order Ba line at 4553·4 is not marked in the Fe spectrum.
4489 ·5 Y 15	3	4489 ·5 Fe 4489 ·5 Mn (3)	Fe line omitted from Thalén's table, but coincidence marked in map. Being stronger in Mn than in Fe, it cannot be due to an impurity of the latter, and as the second order Mn lines at 4498·2, 4472·4, and 4470·5, are not marked in the Fe spectrum, it cannot be due to Mn in Fe. Therefore, this cannot arise from impurity.

Wave-length of line and number of times seen by Young.	Intensity in Sun.	Metallic coincidences, Thalén, and Intensities.	Remarks.
4426·8	3	4426·8 Fe 4426·8 Ti (1)	Fe line omitted from Thalén's tables; coincidence marked in map. Being faint in Fe and strong in Ti, it cannot be due to Fe in Ti. Neither can it be due to Ti in Fe, as the other first order Ti line at 4413·0 is not in Fe spectrum. This line, therefore, does not arise from impurity.
4414·7 Y 1	1	4414·7 Fe (1) 4414·7 Mn (2)	This line being stronger in Fe than in Mn cannot be due to an impurity of Mn in Fe. Neither can it be due to an impurity of Fe in Mn, as the equally strong Fe lines at 4404·2 and 4382·8 are not shown in the Mn spectrum. This, therefore, does not arise from impurity.
4407·7 Y 1	4	4407·7 Fe 4407·7 Ca (5)	Fe line omitted from Thalén's tables; coincidence marked in map. First order Ca line at 4425·0 not in Fe spectrum; nor first order Fe line at 4404·2 in Ca. This, therefore, does not arise from impurity.
4401·7	3	4401·7 Fe 4401·7 Ni (5)	Fe line omitted from Thalén's tables; coincidence marked in map. No strong Ni lines near.
4379·1 Y 1	4	4379·1 Fe 4379·1 Ca (4)	This line cannot arise from impurity for same reasons as line at 4407·7.
4307·0 Y 3	1	4307·2 Fe (1) 4307·5 Ti (5)	
4302 Y 3	4	4302·3 Fe 4302·3 Ca (1)	Fe line omitted from Thalén's tables; coincidence marked in map. Being faint in Fe and strong in Ca, it cannot be due to an impurity of Fe in Ca. The nearest long Ca line 4226·3 not in Fe spectrum; therefore this cannot arise from impurity.
4298·5	4	4298·5 Ca (3) 4298·5 Fe (4)	This line cannot be due to an impurity of Fe in Ca, as first order Fe line at 4307·2 is absent from Ca spectrum; neither can it be due to an impurity of Ca in Fe, for first order Ca line at 4302·3 is absent from Fe spectrum. This line, therefore, does not arise from impurity.
4293·8	3	4293·9 Fe (4) 4293·8 Ti (5)	
4287·0	4	4286·0 Fe (4) 4287·0 Ti (5)	In Thalén's map this is marked as a coincidence at wave-length 4287·0, while in the tables the Fe line is put down as 4286·0.
4271·5	1	4271·3 Fe (1) 4271·5 Ca (5)	

Wave-length of line and number of times seen by Young.	Intensity in Sun.	Metallic coincidences, Thalén, and Intensities.	Remarks.
4253·9	3	4253·9 Fe 4253·9 Ca (5) 4253·9 Cr (1)	Fe line coincident according to map, but omitted from Thalén's tables. This line being so much more intense in Cr than in Fe or Ca, it cannot be due to an impurity of either of the last metals in Cr. It is not easy, however, to prove that it is not due to an impurity of Cr in Ca, as the other two long Cr lines in this region are also given to Ca by Thalén.
4249·8	1	4249·8 Fe (1) 4249·8 Ca (4)	This line being feeble in Ca cannot be due to an impurity of Ca in Fe, but being one of the strongest Fe lines of this region, it may or may not be due to an impurity of Fe in Ca.
4247·5	3	4247·5 Fe (4) 4247·5 Ca (5)	This line cannot be due to an impurity of Fe in Ca, as the first order Fe line at 4250·5 is not in the Ca spectrum; neither can it be due to an impurity of Ca in Fe, as the very intense Ca line at 4226·3 is not present in the Fe spectrum. This line, therefore, cannot arise from impurity.
4237·5	4	4237·5 Fe 4237·5 Ca (5)	" " " " Fe line omitted from Thalén's table; coincidence marked in map.
4233·0 Y 15	4	4233·0 Fe (3) 4233·0 Ca (5)	This cannot arise from impurity for same reason as 4247·5.
4226·8	5	4226·8 Fe (5) 4227·0 Mn (1)	
4184·6	3	4184·6 Fe 4185·0 Ti (3)	This is a coincidence according to Thalén's map at wave-length 4184·6, while according to the tables the Ti line is 4185·0, the Fe line being omitted.
4171·0	3	4171·0 Fe 4171·0 Ti (1)	Coincidence given in map. Fe line omitted from tables. This cannot be due to an impurity of Fe in Ti, being faint in the former and strong in the latter. There is no proof that it may not be due to an impurity of Ti in Fe.
4143·1	1 or 2	4143·1 Fe (1) 4143·0 Ca (4)	
4131·5	3	4131·5 Fe (1) 4131·5 Ca (4)	There is not sufficient evidence to show that this line is not due to an impurity of Fe in Ca. It cannot be due to an impurity of Ca in Fe, being so much stronger in the latter.

Wave-length of line reversed.	No. of reversals seen by Young.	Intensity of line in Sun.	Metallic coincidences (Thalén) and intensities.	Nearest strong line of each metal.	Whether observed in chromosphere. If so, how many times.	
		1 = darkest.	1 = brightest.	1 = brightest.		
3967·9	75	1	3968·0 Ca (1)			
3932·8	50	1	3932·8 Ca (1)			
5172·0	50	1	5172·0 Mg (1)			
5183·0	50	1	5183·0 Mg (1)			
5534·1	50	4	5534·5 Ba (1)	No long Ba line near.		
			5533·5 Sr (2)	5522·5 Sr (2)	No	
5889·0	50	1	5889·0 Na (1)			
5895·0	50	1	5895·0 Na (1)			
4215·3	40	2 or 3	4215·3 Sr (1)			
			4215·3 Ca (2)	4226·3 Ca (1)	Yes	3
4923·1	40	3	4923·1 Fe (3)	4956·7 Fe (1)	Yes	1
			4923·0 Sn (4)	4858·0 Sn (3)	No	
5168·3	40	1 or 2	5168·3 Fe (3)	5166·7 Fe (2)	Yes	30
			5168·3 Ni (5)	5034·6 Ni (3)	No	
5525·9	40	3	Apparently belongs to Fe, but there is a mistake in either map or tables.			
4235·5	30	2	4235·5 Fe (3)	4249·8 Fe (1)	No	
4245·2	30	3	Apparently belongs to Fe, but there is a mistake in map or tables.			
4899·3	30	5	4899·3 Ba (2)	4933·4 Ba (1)	Yes	30
4921·3	30	4	None.			
4933·4	30	3	4933·4 Ba (1)	No long Ba line near.		
5015·0	30	Not in Ångström's map.	5015·3 Ti (2)	5013·3 Ti (1)	No	
5017·6	30	2	None.			
5166·7	30	1	5166·7 Mg (1)	5172·0 Mg (1)	Yes	50
			5166·7 Fe (2)	5226·2 Fe (1)	Yes	10
5275·0	30	3	None.			

Wave-length of line reversed.	No. of reversals seen by Young.	Intensity of line in Sun.	Metallic coincidences (Thalén) and intensities.	Nearest strong line of each metal	Whether observed in chromosphere. If so, how many times.	
		1 = darkest.	1 = brightest.	1 = brightest.		
4077·0	25	3	4077 Ca (3)	3968·0 Ca (1)	Yes	75
5424·5	25	Not in Ångström's map.	5425·0 Ti (3) 5425·0 Ba (3)	5428·6 Ti (2) 5534·5 Ba (1)	5428·8 5534·1	8 50
6140·6	25	2	6140·6 Ba (1)	No long Ba line near.		
6676·9	25	5	None.			
4468·5	20	3	4468·5 Ti (1)	4443·0 Ti (1)	Yes	10
4490·9	20	5	4491·0 Mn (5)	4498·2 Mn (2)	No	
4918·2	20	2	4918·2 Fe (2)	4919·8 Fe (1)	No	
5283·4	20	Not in Ångström's map.	5282·8 Ti (1) ?	5296·7 Ti (1)	No	
5361·9	20	3	5361·9 Fe (4)	5370·5 Fe (1)	Yes	10
6429·9	20	..	Not in Thalén's tables.			
4233·0	15	4	4233·0 Ca (5) 4233·0 Fe (3)	4226·3 Ca (1) 4249·8 Fe (1)	Yes No	3
4394·6	15	4	None.			
4489·4	15	4	4489·5 Mn (3) Marked in map as Fe also, but not in Thalén's tables.	4498·2 Mn (2)	No	
4500·3	15	2	4500·7 Ti (1)	4526·1 Ti (1)	No	
4583·2	15	3	None.			
4629·0	15	3	4629·0 Ti (3)	4638·8 Ti (1)	No	
5197·0	15	5	None.			
5269·5	15	2	5269·5 Fe (1) 5269·4 Ca (2)	5268·5 Fe (1) 5348·6 Ca (2)	Yes No	12
5518·7	15	4	This seems to be a Ba line, but there is a mistake in tables or map.			
5661·5	15	3	5661·5 Fe (3)	5657·6 Fe (1)	No	
6515·5	15	2	None.			

direction, since, as I have been anxious to avoid any question arising from the known impurity of carbon poles, I have limited myself to the use of double metallic poles or experimented on very volatile substances, the impurities carried up by which are easily detected. I append a provisional table of the results I have already obtained. I do not hold to its absolute accuracy, but I know it is sound in the main. (See p. 443.)

The dispersion I have employed (Rutherford's grating, 17,000 lines second order) has been so great that the observations would have been more satisfactory if the light of the arc had been less reduced by dispersion. We may therefore feel assured that we are dealing with lines of the same wave-length, identical, that is, within the range of the most powerful instrumental methods ordinarily employed. In all cases a battery of thirty Grove cells was used, and the lines were successively observed at the intersection of two cross-wires in the field of view, everything remained unchanged and rigid except the poles between which the arc was made to pass. For the lines between H and G an inspection of photographs has taken the place of eye observations, and for this not only my own series of photographs has been used, but a most valuable one placed at my disposal by Professor Roscoe.

The list in its present very incomplete state leads to very remarkable conclusions; the line at 1474 has been found in several spectra, while Lorenzoni's *f* is markedly absent from the spectra of forty-two metallic elements; this result, I think, justifies the suspicion long ago stated that *f* belongs to the same substance, or at least is nearly related to that which produces D_3. It will be seen too that a line coincident with the *h* line of hydrogen has been photographed in the spectra of several substances besides indium, G being absent, and F also, as I have gathered from an inspection of Dr. Roscoe's photographs. I may add that I have reason to suspect the existence of the C line alone in the spectra of some chemical substances.

These observations have compelled me to make rapid surveys of the arc-spectra of most of the metallic elements, and have again brought to the front, in a very striking way, the view I expressed to the Royal Society some five or six years ago, that many of the lines in line-spectra are the brightest portions—the remnants—of flutings and possibly of other rhythmic structure. I am at present engaged in investigating this question of rhythm, and I have already found that many of the first order lines of iron may probably arise from the superposition or integration of a number of rhythmical triplets. All this goes to show how long the series of simplifications is that we bring about in the case of the so-called elementary bodies by the application of a temperature that we cannot as yet define. Indeed the more one studies spectra in detail, and especially under varying

Wave-length	4010	4340	4471	4861	5166·7	5168·3	5172	5183	5315·9	5534·1	4933	6140·6	5269
Usual designation	*h*	*G*	*f*	*F*	b_4	b_2	b_3	b_1	"1474"				
No. of times given by Young	100	100	100	100	30	40	50	50	90	50	30	25	15
Ce, La, Di	+	−	−	+	−	−							
In	+	−	−	−	−								
Ru	+	*p*	−	−									
Sn	+	*p*	−	−									
U	+	+	−	−									
Fe	−	−	−	−	+				+	+	+		+
Mo	−	−	−	−	+				+				
W	−	−	−	−	+				+				
Co	−	−	−	−	+				+	+			
Mn	−	−	−	−	+								+
Ca	−	−	−	−	+				−	+	+	+	+
Li	−	−	−	−	*d*								
Na	−	−	−	−	*d*				−	+	+		
Cu	−	−	−	−	+				−	+			
Al	−	−	−	−	+				+				+
Ba	−	−	−	−					−	+	+	+	+
Sr	−	−	−	−					+	+	+		
Au	−	−											
Ag	−	−	−	−						+	+	+	
Ti	−	−	−	−					+	+		+	
Ni	−	−	−	−		+			+			+	
Cr	−	−	−	−					+				+
Sb	−	+	−	−									
As	−		−	−									
Bi	−	−	−	−									
Pt	−	−	−	−									
Cs	−	−	−	−									
Gl	−		−	−									
Ir	−		−	−									
Pb	−		−	−									
Mg	−		−	−	+		+	+					
Yt	−												
Er	−												
Os	−		−	−									
Pd	−	−	−	−									
K	−	−	−	−									
Rb	−	−	−	−									
Tl	−	−	−	−									
Sn	−	−	−	−									
U	−		−	−									
Va	−	−	−	−									
Zn	−		−	−									
Hg	−	−	−	−									
Ru	−												
Rh	−												
Ta	−												
Zr	−												
Nb	−												
Th	−												

+ means present. — means absent. *p* means probable. *d* means doubtful.

conditions of temperature which enable us to observe the reversal now of this set of lines, now of that, the more complex becomes the possible origin. Some spectra are full of doublets: sodium and potassium, as ordinarily mapped, may be said indeed to consist exclusively of doublets; others, again, are full of triplets, the wider member being sometimes on the more, sometimes on the less, refrangible side. Doublets and triplets, as a rule, reverse themselves more freely than the irregular lines in the same spectrum—which particular doublet or triplet will reverse depending upon the temperature, as if the cooler vapour to which the reversal is due varied as in the case of fractional distillation. Some lines are clean cut in their reversal; others, again, to use the laboratory phrase, are fluffy to a degree that must be seen to be appreciated, so much so, that when photographed they appear merely as blurs upon the plate.

The above results, which have been foreshadowed in my previous papers, have led me to examine especially the intensities of the various Fraunhofer lines, and to compare the intensities of the metallic lines confronted with them in arc and sun photographs. I have done this because it is worse than useless to proceed with this construction of the large map now that four years' work has shown that the method of impurity elimination has proved insufficient, until some other method, embodying a higher law, can be used; and to get this we want work over the whole field. This examination I am making, not only from K to G, over which my own photographs extend, but even to *b*, by means of another series taken by Professor Roscoe, which he has allowed me to inspect.

In short, in this survey I have about 300 photographs to work upon. I exhibit several of these photographs to the Society in anticipation of a further communication.

The upshot of this inquiry even already is as follows:—The discrepancy which I pointed out, six years ago, between the solar and terrestrial spectra of calcium is not an exceptional, but truly a typical case. Variations of the same kind stare us in the face when the *minute anatomy* of the spectrum of almost every one of the so-called elements is studied. If, therefore, the argument for the existence of our terrestrial elements in extra-terrestrial bodies, including the sun, is to depend upon the perfect matching of the wave-lengths and intensities of the metallic and Fraunhofer lines, then we are driven to the conclusion that THE ELEMENTS WITH WHICH WE ARE ACQUAINTED HERE DO NOT EXIST IN THE SUN.

March 27, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Organization of the Fossil Plants of the Coal Measures. Part X." By W. C. WILLIAMSON, F.R.S., Professor of Natural History in Owens College, Manchester. Received March 5, 1879.

(Abstract.)

The still existing differences of opinion respecting the botanical affinities of the Sigillariæ give value to every new fact calculated to throw light upon the question. In 1865, Edward Wunsch, Esq., of Glasgow, made a discovery, which proves to have an important bearing upon it. He found, at Laggan Bay, in Arran, a series of rather thin Carboniferous strata, separated by thick beds of volcanic ash, and in one of the Carboniferous shales especially, he discovered the bases of the stems of numerous very large trees, standing perpendicularly to the shales. These trees have been referred to by several authors as Sigillarian. In the summer of 1877, Mr. Wunsch and I employed quarrymen to make extensive excavations amongst these strata, for the purpose of adding to the extensive series of specimens which he had obtained, and the whole of which he kindly placed in my hands. The aggregate result of these explorations was to show that the conclusion previously arrived at, viz., that the stems had belonged to a grove of Sigillarian trees was unsupported by a solitary fact. These stems were of very large size, showing that they had belonged to fully grown trees. None of them displayed any traces of leaf-scars, having outgrown the stages at which such scars would remain visible. Their outer surfaces were scored with deep irregular longitudinal fissures, resulting from internal growth and consequent expansion, and which appear to have been mistaken for the longitudinal grooves and ridges of a Sigillarian bark. Such, however, they certainly were not, since, in every instance, the surface bark had been entirely thrown off, and the fissures entered deeply into the subjacent bark layer. In most of the stems, this comparatively thin bark layer was the only one that remained, the greater portion of the inner bark and the central vascular axis having disappeared, leaving a large cylindrical cavity, which was

filled up with volcanic ash. These stems failed to display a single feature, justifying the conclusion that they were Sigillarian.

In two of them the central cavity, instead of being filled with ash, was filled with miscellaneous heaps of vegetable matter, amongst which were large fragments of the vascular axes of various plants, such as *Lepidodendra* and *Stigmariæ*, but in one of the largest stems were five or six decorticated vascular cylinders of Diploxyloid stems, of the largest size, and which, though arranged parallel to the long axis of the cylinder which enclosed them, obviously did not belong to them, but had been floated in from without. The supposition that these had been young stems that had grown within the hollow protecting cylinders, from spores, accidentally introduced, is wholly untenable, since each one of these several vascular axes has been the centre of a stem fully as large as that within which we found them aggregated. Of course, these Diploxyloid vascular axes had the organization which Brongniart and the younger school of French botanists which still upholds his views on this point, believe to be characteristic of true Sigillariæ—a conclusion from which I have long dissented.

The only fragment we found, that threw any light upon the character of the leaf-scars that had indented the surfaces of these fully grown stems, was a well-defined example of the Lepidodendroid type.

We directed careful attention to the nature of the smaller fragments of branches and foliage which abounded in the volcanic ash with which the large stems were overlaid. These consisted of Lepidodendroid branches and twigs of all sizes and ages, and no doubt was left upon my mind that they were really the *disjecta membra* of the stems around which they were so profusely scattered. The only fruits that have been obtained from the same locality are *Lepidostrobi*, most of which contain macrospores and microspores. Unless we are prepared to believe that this Arran deposit contained, on the one hand, numerous stems without branches, and, on the other, yet more numerous branches without stems, we must recognise in these specimens the complementary elements of a grove of Lepidodendroid trees.

One specimen found is a very important one. It has a mean diameter of six inches, and is either a small stem or a very large branch. Internally it exhibits the same structure as all the smaller Lepidodendroid branches, except so far as it is modified by size and age. But in addition to its other features, it exhibits *a very narrow* exogenous ring surrounding the ordinary Lepidodendroid one, thus giving some clue to the size attained by such branches before the internal organization passed from the Lepidodendroid to the Sigillarian type.

I have at last succeeded in obtaining the Strobilus, to which the remarkable macrospores and microspores figured in my last memoir

belong. It unexpectedly proves to be a very small one, being little more than an inch in length. Further specimens have shown that the abnormal peduncles of the macrospores shown in Plate 23, fig. 64, are wholly due to the partial collapse of the spore-wall. Further specimens have also been obtained of the Strobilus and its spores represented in Plate 22, figs. 38–57. These examples possess the central vascular axis in a perfect state, which portion was lacking in the previously known examples. It proves to have an individuality as distinctive as that of the spores and sporangia which it bore.

The important discovery by Mr. D'Arcy Thompson, of Edinburgh, of young branches of *Ulodendron* with reproductive cones actually attached to the scars characteristic of the genus, finally settles the nature and functions of these scars, showing that they mark the positions from which bilaterally arranged deciduous organs of fructification have fallen.

The structure of *Calamostachys Binneyana* has had further light thrown upon it, sustaining my previously expressed convictions that it had a triquetrous axis, and that consequently its affinities were with *Asterophyllites* and *Sphenophyllum*, and not with *Calamites*. A specimen demonstrates that the six vascular bundles going to the six fertile sporangiophores were given off in pairs from the three truncated angles of a triangular vascular axis—an orientation absolutely identical with that represented in similar sections of stems of *Sphenophyllum*, published by M. Renault. The recent discovery by Herr Stur, of Vienna, of a plant in which Sphenophylloid and Asterophyllitean leaves are found upon a common stem, establishes the correctness of my previous conclusions as to the very close affinities of these two genera.

Two new fern petioles or stems have been obtained from Halifax, to which I have given the name of *Rachiopteris robusta* and *R. insignis*. In one specimen of the latter, the large vessels of the central bundle are full of Tylose cells, whilst a second example exhibits no trace of them. This shows the existence or non-existence of Tylose to be a characteristic having no specific value.

Since my last memoir was written I have obtained several new forms of cryptogamic conceptacles—similar to those previously described under the generic name of *Sporocarpon*—as well as been able to throw additional light upon some of those previously described. No clue has yet been obtained as to the plants to which these very remarkable organisms belonged.

A large series of specimens from Oldham and Halifax has enabled me to investigate in detail the very curious objects to which Mr. Carruthers gave the name of *Traquairia*, and which that observer believes to be a form of Radiolarian life. Their very elaborate organization can scarcely be made intelligible without the aid of plates. In a

previous memoir ("Phil. Trans." 1874, p. 56), I ventured to doubt the correctness of Mr. Carruthers' conclusions, and expressed my conviction that these objects resembled spores rather than protozoan skeletons. Further study of their details of structure has only strengthened this opinion which has also received the important support of Professors Hæckel and Strasburger, of Jena, both of whom have carefully studied my collection of specimens. These objects are small spheres—the sphere-wall of which is prolonged into a series of long radiating tubes not unlike the muricated spines of a *Cidaris*. In their young state each murication gives off a delicate thread or threads, which ramified freely in an apparently mucilaginous or gelatinous, structureless, investing magma. In older specimens these threads developed into branching and radiating cylindrical tubes which, like the primary ones, had very thin walls. Within the outer sphere-wall, which consists of the coalesced bases of these branching tubes, were at least two other thin layers of membrane, and in several of the specimens the interior of the capsule is filled with cells, exactly like those seen in the corresponding cavities of Lycopodiaceous macrospores found in the Halifax deposits from which the finest *Traquairiæ* have been obtained. These objects differ considerably from all known reproductive structures; but I agree with Professor Hæckel in his very decided rejection of them from the Radiolarian group of organisms, and with his conclusion that they are vegetable and not animal structures. Professor Strasburger thinks it most probable that their affinities are with the macrospores of the Rhizocarpæ.

In my previous memoir I gave three very small figures of some minute objects, which exactly resemble, in their minutest details, the zygospores of some of the Desmidiaceæ. Many additional examples of these objects have been discovered, enabling me to throw further light upon them. Their resemblance to these zygospores has been made increasingly obvious, but I dare not venture to assign to them a Desmidiaceous origin, since the most extended research, and the resulting discovery of large numbers of these organisms, have yet failed to bring to light the faintest trace of a true Desmid. Under these circumstances I have assigned to several species of these organisms the generic name of *Zygosporites*.

The seed described in my last memoir but one, under the name of *Lagenostoma ovoides*, always exhibited a thick carbonised testa, in which no structure could be observed. I have now discovered that the thick outer layer consisted of very hard cubical or slightly oblong schlerenchymatous cells, whilst a thin and delicate inner membrane was composed of small spiral prosenchymatous ones.

An additional specimen of the woody axis of *Dadoxylon* exhibits the paired divergent structures passing outwards to the back in the shape of two large, radial prolongations of the cellular pith; and which

must obviously have gone off the branches—either to ordinary ones or to pairs of fruit-spikes.

Myriads of the vegetable fragments both from Oldham and Halifax are drilled in all directions with rounded insect or worm borings, and further traces of these xylophagous animals are seen in innumerable clusters of small Coprolites of various sizes; the size of those composing each cluster being uniform.

Desirous of verifying Count Castracane's alleged discovery of Diatoms in coal, specimens of twenty-two examples of coal from various localities in Yorkshire, Lancashire, and Australia were reduced, after the Count's method, to a small residue of ash. This work was done for me in the chemical laboratory of Owens College through the kindness of Professor Roscoe. Like Mr. F. Kitton, of Norwich, the Rev. E. O'Meara, of Dublin, and the Rev. G. Davidson, of Logie Coldstone, I have failed to discover the slightest trace of these organisms in coal.

The last objects described are some minute organisms from the Carboniferous limestones of Rhydmwyn, in Flintshire, and which were supposed by Professor Judd to have been siliceous Radiolarians from which the silica had disappeared and been replaced by carbonate of lime. I fail to find any confirmation of this conclusion. The objects appear to me to constitute an altogether new group of calcareous spherical organisms that may either have been allied to the Foramnifera, or have had some affinities with the Rhabdoliths and Coccoliths. I have proposed for several species of the organisms the generic name of *Calcisphæra*. Myriads of objects of similar character, but of larger size, constitute the greater portion of a Corniferous limestone from the Devonian beds of Kelly's Island, U.S.A.

II. "Observations on the Physiology and Histology of *Convoluta Schultzii*." By P. Geddes. Communicated by J. Burdon Sanderson, M.D., F.R.S., Professor of Physiology in University College, London. Received March 10, 1879.

Part I.—*Physiology*.

Chlorophylloid green colouring matters are known to exist in the tissues of a not inconsiderable number of animals belonging to very various invertebrate groups—Protozoa, Porifera, Cœlenterata, Vermes, and even Crustacea;* but all information as to the function of chlorophyll in the animal organism is wanting. Wöhler, it is true, found many years ago that *Chlamydomonas*, *Euglena*, &c., evolve oxygen in sunlight, and Schmidt prepared from *Euglena viridis* a body isomeric

* See list in Sach's "Botany," Eng. ed., p. 687, note.

with starch, though of widely different properties, his paramylon; * but these facts seemed as much to point towards the algoid nature of these long disputed organisms † as to warrant our supposing a more or less vegetable mode of life in animals so well organised, and so evidently carnivorous as Cœlenterates and Turbellarians, especially as the only recorded experiment, that of Max Schultze‡ on *Vortex viridis*, yielded a totally negative result. Some such hypothesis, however, can hardly help recurring to the observer of the light-seeking habit of *Hydra viridis*.

Last spring, when at the Laboratoire de Zoologie Expérimentale of M. de Lacaze-Duthiers, at Roscoff, I was much interested by the green Rhabdocœle Planarian,§ *Convoluta Schultzii*, *O. Schm.*, crowds of which, lying at the bottom of the shallow pools left by the retreating tide, resembled at first sight patches of green filamentous algæ. Their abundance in fine weather on the surface of the white sand, covered only by an inch or two of water apparently to bask in the sun, was very striking, at once suggesting that their chlorophyll thus so favourably situated must have its ordinary vegetable functions. I accordingly returned to Roscoff in the autumn to make experiments.

The mode of procedure was evidently to expose the Planarians to sunlight to observe whether any gas was evolved, and if so to analyse it qualitatively and quantitatively. After one or two trials a form of apparatus—the simplest possible—was found, which answered admirably. It merely consisted of a couple of the round shallow glass dishes used in the laboratory as small aquaria, the edge of one fitting as nearly as possible, when inverted, into the bottom of the other. Into the larger vessel were put Planarians enough to cover the bottom; it was then gently sunk in the pneumatic trough (a tub of sea water), and the smaller, also full, inverted into it. The apparatus was then placed on a shelf in the sunshine, and left to itself. The movements of the animals were greatly accelerated by the exposure, and in a quarter of an hour minute bubbles of gas were to be seen in the film of mucus plentifully secreted by the Planarians. These bubbles rapidly increased in number and volume until they buoyed up the whole sheet of mucus with its entangled Planarians and grains of sand to the top of the water in the inverted dish. Here the evolution of gas continued more actively than ever, until the animals had disengaged themselves and descended to the bottom, there to recommence as before, the mucus meanwhile dissolving and allowing the bubbles freely to unite. Thus the first half of the inquiry was answered in the affirmative.

* Gorup Besanez, "Traité d'Analyse Zoochimique," p. 127.

† *Euglena* is claimed by both Sachs and Claus in their manuals of Botany and Zoology respectively.

‡ "Beiträge zur Naturgeschichte der Turbellarien."

§ "*Neue* Rhabdocœlen." Wiener Sitzungsb., 1852.

The determination of the nature of the evolved gas was readily effected. On transferring the quantity produced in one or two vessels to a small test-tube, and plunging into it a match with red hot tip, there was to be seen the white glow characteristic of dilute oxygen. A large glass tube of tolerably even calibre, about 75 centims. long, was sealed at one end, and bent at about two-thirds of its length from that point at an angle of 60°. It was then filled with water, and the water in the long sealed arm almost entirely replaced by gas at the pneumatic trough. This comparatively large quantity of gas, about 60 centims. cube, was obtained by exposing a dozen or so of apparatuses exactly similar to that described, except that bell-jars, sealed funnels, &c., sometimes replaced the upper flat dish, and white soup plates the lower. They were set agoing about noon, and the abundant gas yielded by thus exposing a surface of nearly a third of a square metre covered with Planarians was collected at sunset.

On agitating the gas with a solution of potassic hydrate a barely appreciable absorption of carbonic anhydride took place, but on the addition of pyrogallic acid with renewed agitation, the intense brown coloration, with rapid and considerable ascent of the fluid in the long arm of the tube, confirmed the presence of a large percentage of oxygen.

The results of many experiments varied from 43 to 52 per cent. of oxygen; the higher number representing the amount of gas given off by freshly collected Planarians, and the lower that yielded on the second or third day of their subjection to experiment. In order to judge of the degree of accuracy which I could obtain by this rough method of analysis, I estimated by it the oxygen of common air, and obtained 19·9 per cent. instead of 20·9. Allowing for this loss of about 5 per cent., it may safely be asserted that the gas evolved by these animals does not contain less than from 45 to 55 per 100 of oxygen.

The Planarians are little the worse after a 24 hours' journey from Roscoff to Paris, and when placed in an aquarium they instantly betake themselves to the side next the window, and live there resting on the bottom or clinging to the side for four or five weeks without food. They certainly diminish considerably in size, yet I have little doubt that they go on decomposing CO_2 and assimilating the carbon even in the dull winter daylight, for when kept in darkness they generally died much sooner.

The conspicuousness of the Planarians on the sandy beach, far from the shelter which rocks or algæ might afford, has been already mentioned, and at first sight one is apt to think that they must be the easy prey of all the larger shore-frequenting animals, and to wonder that so many escape. But the observation made by Wallace and Belt for so many higher animals—that conspicuously coloured forms are

nauseous and uneatable—holds good here. So strong and disagreeable is the odour, to which the taste doubtless corresponds, that this alone might be relied upon as a protection against the least fastidious of fishes or Crustaceans.

The chemical examination of the animal yields results of interest. Treated with alcohol, a yellow substance, contained in small elongated vesicles, aggregations of which are dotted over the integument, dissolves out very rapidly, yielding a golden solution without definite spectrum. This has of course nothing to do with xanthophyll. Continued treatment with alcohol dissolves out the chlorophyll, of which the magnificent green solution is tolerably permanent. As former observers have shown, it has a red fluorescence, and gives a spectrum closely resembling that of vegetable chlorophyll.

Knowing that these animals decompose carbonic acid, and evolve oxygen, one naturally enquires whether they do not still more completely resemble green plants in fixing the carbon in the same way. To answer this question, the residue of the Planarians, coagulated and decolorised by repeated treatment with alcohol and ether, was boiled with water, and filtered off. The clear solution gave with iodine solution a deep blue coloration, which disappeared on heating, and reappeared on cooling, indicating the presence in quantity of ordinary vegetable starch.

To separate and purify this starch on a large scale, some hundred grammes of Planarians were repeatedly boiled in water. The solution (which had an intensely alkaline reaction) was treated with four or five times its bulk of strong alcohol, and allowed to stand for some days. The flocculent precipitate was collected, decolorised with ether, and washed with cold water. A great part of it dissolved, leaving the starch behind, and the filtered solution gave with iodine the red-brown coloration characteristic of dextrine. To ascertain whether this dextrine was naturally present, or had merely been produced at the expense of the starch by boiling in alkaline solution, fresh animals were treated with cold water, but the solution contained no dextrine. Treatment of a fresh microscopic preparation with iodine showed the presence of glycogen, in the colourless amœboid cells of the mesoderm, but there is no chemical means of separating glycogen from starch. Probably the best way of obtaining pure starch from these animals would be by imitating the mechanical process of the potato mill.

The intense alkalinity of the animals is very striking. Even in the fresh state, but still more when dried in the warm chamber, they give off vapours with an odour resembling that of trimethylamine, and in such abundance as to cause neighbouring solutions to yield the reactions of an alkaloid. A quantity of animals was distilled, and the alkaline fumes received in dilute hydrochloric acid. The resultant salt

was purified by repeated solution and recrystallization in absolute alcohol. With $PtCl_4$ it yielded a precipitate, which was kindly analysed for me by Dr. Magnier de la Source, and found to be the platino-chloride of methylamine: however, it is very probable that the volatile alkaloid was really more complex, but broke up in the distillation. The subject would repay the attention of a chemist. Trimethylamine has been obtained from many animal sources, and the production of this, or some nearly allied body, in such remarkable quantity by *Convoluta* seems to be a protective specialisation.

The ash of the *Convoluta* contains iodine, another analogy to the algæ.

As the *Drosera*, *Dionæa*, &c., which have attracted so much attention of late years, have received the striking name of Carnivorous Plants, these Planarians may not unfairly be called Vegetating Animals, for the one case is the precise reciprocal of the other. Not only does the *Dionæa* imitate the carnivorous animal, and the *Convoluta* the ordinary green plant, but each tends to lose its own normal character. The tiny root of the *Drosera* and the half-blanched leaves of *Pinguicula* are paralleled by the absence of a distinct alimentary canal and the abstemious habits of the Planarian.

It still remains to ascertain the behaviour of other green animals, and I hope to begin with *Hydra* and *Spongilla*,* as soon as the season permits.

Part II.—*Histology.*

The general characters of the animal have been already given by Schmidt, and I need only add that I have succeeded in making out the mouth, which lies, as usual in this genus, a little way behind the otolith. It is not a mere transverse slit, but is surrounded by a lip capable of slight protrusion, which evidently corresponds to the protrusible pharynx of higher Planarians. When feeling its way the animal has a curious habit of sharply retracting the terminal point of the anterior ends of the body, the head thus becoming bilobed, with a central depression. Each lobe becomes a sort of temporary tentacle, and these may be compared with the blunt permanent head lobes of allied forms. So too the animal "when extremely contracted" throws its smooth dorsal integument, not into irregular wrinkles, but into rounded papillæ, which remind one of the permanent dorsal papillæ of other Planarians.

I will first notice an interesting point in the histology of the ciliated ectoderm. In teased preparations, kept cold, the ciliated cells often become amœboid, some of the cilia changing into slender finger-like or stout fusiform pseudopodia. These often retain their curvature parallel

* Sorby has suggested the probably partial vegetal mode of life of *S. viridis*, and resultant analogy to *Dionæa*. ("Quart. Journ. Micro. Sci.," 1875, p. 51.)

to the unaltered cilia, and I have even seen the finer pseudopodia contracting gently in time with the cilia of the same cell, thus establishing a complete gradation between the rhythmically contractile cilium and the amœboid pseudopodium through what is really a rhythmically contractile pseudopodium. Hæckel and others have accumulated many instances of the transformation of ciliary movement into amœboid and *vice versâ*, but I only know of one case in which the passage-form, the cilium-like pseudopodium, has been actually observed. Lankester,* speaking of developing spermatozoa of *Tubifex*, describes "very large active fusiform masses, exhibiting very rapid movements like a cilium, and possessing at the same time the character of a pseudopodium." It is important that Lankester's passage-form occurred during the transformation of amœboid movement into ciliary, while I find exactly the same thing during the reverse change; and it is not improbable that such ciliary pseudopodia may transitorily occur in many cases.

Perhaps no animal structure has received more varied and contradictory interpretations than the rod-like bodies (*Stäbchen, baguettes*) of the Planarian integument. "Max Schultze holds them for end-organs of nerves, Leuckart and many others for nettle-capsules, Schneider for *spicula amoris*, Keferstein for mucous glands, Graff for more or less developed nematocysts."† Two distinct kinds of organ exist in *Convoluta* and other Rhabdocœles, and have been confused under the same name; first, the heap of coloured rod-shaped bodies, the original "Stäbchen" of Max Schultze, which furnish in *Convoluta* the yellow solution already referred to, and, secondly, large and long spindle-shaped bodies, generally arranged singly, each containing a sharp brittle needle, of which the point lies close under the apex of the spindle. In a teased preparation they are generally empty, showing the tube in which the arrow lay, and with a little granular protoplasm hanging round the mouth like the smoke of the explosion. The dart is generally propelled for some little distance, but sometimes sticks in the mouth of the tube. Graff's view ‡ is certainly the right one, that these are offensive weapons, but they are constructed on so distinct a plan from those of Cœlenterates, that they might better be called sagittocysts than nematocysts. True nematocysts have been described in some other Planarians.

Below the epidermis lie the circular and longitudinal muscles, and beneath them comes the layer of chlorophyll-containing cells. These are clear and semi-fluid, more or less irregular in shape, but becoming spherical when separated. The chlorophyll is not collected into granules as in the higher plants, nor into drops as in the green cells of *Vortex viridis*, but is diffused throughout the whole pro-

* "Quart. Journ. Micro. Sci.," 1870, p. 292.

† Minot, "Studien an Turbellarien," "Semper's Archiv," III, 4, 1877.

‡ "Zeitsch. f. w. Zool.," xxv, p. 421.

toplasm of the cell, which is thus very intensely coloured. One, or sometimes two, nuclei are present, besides an irregular heap of granules. It was very difficult to break up the cell completely, and so liberate the granules, but in one or two fortunate preparations treated with iodine, the blue coloration assumed by many of these granules proved that we have here an actual deposit of starch, quite like that which Sachs has shown to take place within the chlorophyll granules of the plant. These starch granules are many of them so minute as to show Brownian movements; the larger are quite amorphous, and consequently exhibit no polarisation.

Deeper than the green layer, lie colourless granular nucleated cells, which may be spherical or branched. These yield with iodine the red-brown reaction of glycogen very conspicuously indeed. All the internal tissues of the animal are bathed in that abundant slimy protoplasm which has been so often adduced in evidence of the infusorian affinities of the lower Turbellaria. It exudes from all points of the body of a squeezed *Convoluta* in hyaline drops, which generally enclose a heap of cells of all sorts, and which often show amœboid movements. This semi-fluid protoplasm oozing through the loose cell meshes with every movement of the body may well serve instead of a special circulatory fluid. Digestion may also be effected by the amœboid protoplasm, for it is easy to confirm the statements of Claparède, Metschnikoff,* Ulianin,† and Graff,‡ as to the absence of any distinct alimentary canal.

The development of the generative products is of interest. An apparently ordinary mesoderm cell enlarges and divides into an oval mass of about 12—16 segments. The granular protoplasm of these is gradually drawn out into the very long spermatozoa, and thus each testicular mass is transformed bodily into a bundle of neatly folded spermatic filaments. The ova are also developed by the division of a mesoderm cell. There are no separate vitellaria, but the yolk granules seem to arise in the finely granular amœboid protoplasm of the developing ovum.

The "otolith" is transparent and strongly refracting. It is loosely contained in a capsule and shaped like a plano-convex lens, but with the plane under surface very rugged. I can form no hypothesis as to its function. In some forms what appears to be a nucleus is present, and the body is probably a modified epithelial cell.

Everywhere imbedded in the mesoderm are numerous small colourless cells scarcely so big as a frog's red blood corpuscle. These are more or less pear-shaped, with a large central cavity; and lining one

* "Zoologischer Anzeiger," 1878, p. 387.

† "Die Turbellarien vom Bucht von Sebastopol." Moscow, 1870.

‡ "Kurze Ber. über fortgesetzte Turbellarienstudien," Zeitch. f. w. Zool., xxx, Supp., p. 463.

side of the interior of this cavity and parallel to the long axis of the cell, are a number of distinct transparent homogeneous filaments inserted above and below into the ordinary granular protoplasm which constitutes the remainder of the cell. This division of the cell into a granular and a fibrillated portion is similar, as Dr. Malassez suggested to me, to that which obtains in the developing muscular cell of a tadpole's tail, and though also somewhat remotely, to the structure described by Lankester in the heart of *Appendicularia.** In a teased preparation, some of these cells are easily found in a state of rapid rhythmical contraction, giving as many as 100—180 energetic beats per minute. The form of the cell alters with every pulsation, shortening and broadening like a contracting muscle. This change of form is simply impressed upon the cell body by the contraction of the internal fibres, and does not therefore truly correspond to that observed in a muscle. Some cells also of extreme curvature (for hardly any two are quite alike) bend sharply and return with a spring. The movements soon become slow and inco-ordinate, and waves can be seen passing along the separate fibres independently of each other. The movement stops altogether and the cell bursts, but the fibres resist for some time longer the destructive action of the water.

I have never been able to observe any rhythmical contraction, but at most a feeble quivering within the cell while in the body of the animal, nor to make out any trace of definite arrangement. Max Schultze has described how the alimentary canal of the higher Planarians swarms with *Opalinæ*, and it is possible that these so singular structures may be excessively modified parasitic Infusoria. In any case, the main histological interest lies in the fact that these pulsatile cells cannot be classified either with ciliary or amœboid, with plain or striated muscular cells, but present a distinct type of contractile structure.

In one of these bodies, which had come to rest in the characteristic curved pear-like form, the nucleus-like body, which is often to be seen at one side, was distinctly seen to be in motion. It slowly dived under the contractile filaments, and moved steadily towards the opposite side, displacing the fibres slightly as it pushed its way. When it had reached the middle the cell had straightened into a perfectly symmetrical pear-shape, and by the time it had reached the opposite side the cell had curved to the same side. After a momentary pause it commenced to go back again, and the oscillation of this singular body along the transverse diameter of the cell, with the accompanying changes of form of the whole, continued with perfect steadiness for at least half an hour, enabling me to draw all the phases again and again. One whole oscillation occupied a little over a minute.†

* "Ann. and Mag. Nat. Hist." 1873, p. 88.

† Figures will be published in the "Archives de Zoologie Expérimentale."

I must express my warmest thanks to M. de Lacaze-Duthiers, in whose laboratories at Roscoff and Paris I have received the greatest hospitality. The chemical examination of the animal was conducted in the Laboratoire de Chimie Biologique, of M. le Professeur Gautier, to whom my best thanks are also due and tendered.

Presents, March 6, 1879.

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April 3, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Richard Assheton Cross, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

Pursuant to notice, Arthur Auwers, Luigi Cremona, Jean Louis

Armand de Quatrefages, Georg Hermann Quincke, Theodor Schwann, and Jean Servais Stas were balloted for and elected Foreign Members of the Society.

The following Papers were read:—

I. "On the Thermal Conductivity of Water." By J. T. BOTTOMLEY, Lecturer in Natural Philosophy and Demonstrator in Experimental Physics in the University of Glasgow. Communicated by Professor Sir WILLIAM THOMSON, LL.D., F.R.S. Received March 11, 1879.

(Abstract.)

The experiments described in this paper were undertaken at the instance of Sir William Thomson and by a method devised by him.

The liquid whose thermal conductivity is to be determined is heated from above, to avoid convection currents. Two methods of heating have been used. In one, a horizontal steam chamber is applied at the top of the water or other liquid; and, steam being continuously passed through the heating chamber, the surface of the liquid under experiment is kept at a very high temperature, and heat is conducted from above downwards. In the other method a large quantity of very hot water is deposited on the top of a mass of cold water, mixing being prevented by a simple contrivance; and the heat of this superincumbent layer is conducted downwards through the colder water below.

The experiments have been carried on in very large vessels, or tanks, in order to avoid disturbance by means of loss of heat at the sides. It is intended, at the suggestion of Professor Clerk Maxwell, to observe the loss of heat by the sides under given circumstances, and to estimate, from results of such experiments, the probable error due to this loss.

In the experiments three principal thermometers are employed; together with a fourth, whose object is merely to show when heat begins to be lost at the bottom of the layer of fluid experimented on. When this loss commences the experiment is at an end. The other three thermometers are used thus:—First there is a thermometer with a bulb 30 centims. long. It is placed vertically; and its object is to show the average temperature from top to bottom of the layer of fluid bounded by horizontal planes passing through the top and bottom of its bulb. The rise of this thermometer in any time shows the quantity of heat that has passed into the stratum occupied by it in that time. The other two thermometers are placed with their bulbs horizontal, and one at a known distance vertically above the

other. They indicate the temperatures of the layers in which they are placed.

Now, if we know the difference of temperatures of two sides of a stratum of a liquid during any time, and the quantity of heat conducted across the stratum during that interval of time, we can calculate the thermal conductivity of the liquid by means of a well-known formula.

The result arrived at by the experiments described, is that the thermal conductivity of water may be taken at from ·0022 to ·00245 in square centimetres per second.

Some experiments have been made on the thermal conductivity of solution of sulphate of zinc, a solution which happened to be convenient for preliminary trials. The specific heat of solution of sulphate of zinc at different densities, which it is necessary to know for comparison as to thermal conductivity of that liquid with water, has been determined.

Experiments are now being carried on on this subject with the assistance of a grant from the Government Fund of 4,000*l.*

II. "The Preparation in a State of Purity of the Group of Metals known as the Platinum Series, and Notes upon the Manufacture of Iridio-Platinum." By GEORGE MATTHEY. Communicated by F. A. ABEL, C.B., F.R.S. Received March 19, 1879.

In this paper it is not my intention, nor should I be able, to refer generally to the results of work in the various branches of platinum metallurgy carried out by my firm, who, as is well known, have been associated with the development of this special field of industry from its earliest infancy; but I shall confine myself simply to that section of it upon which my personal attention has of late years been specifically concentrated in order to meet and comply with the requisition of the Bureau Internationale des Poids et Mesures, the Section Françaises de la Commission Internationale du Mètre, and of l'Association Géodésique Internationale (all of them important scientific committees, formed with the object of arriving at an accurate and definite solution of the long agitated question of standard weights and measures), and also at the demand of the French Minister of War, for an alloy the best adapted for the manufacture of the international metre and kilogram standard, and the geodesique rule; and in my endeavour to solve this difficult problem I have had the great advantage of being able to consult those distinguished men, M.M. Henri Sainte Claire Deville and Henri Debray, of Paris, and have also had the

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benefit of the excellent and valued advice of M. Stas, the celebrated Belgian chemist, to all of whom the scientific world owe so much, and to whom I desire to offer my warmest thanks.

In a paper of this kind it would be superfluous for me to enter into any of the already published details concerning the existence and collection of what is known as platinum-dust or mineral. It is sufficient for me to observe that the six metals (of which platinum is the chief) usually found more or less in association in their native state, present characteristics of interest beyond their metallurgical utility, which are, perhaps, worth alluding to *en passant*. It is, for instance, a curious fact that the group should consist of three light and three heavy metals, each division being of approximately the same specific gravity—the heavier being (in round figures) just double the density of the lighter series.

Thus we find osmium, iridium, platinum forming the first division, of the respective specific gravities of 22·43, 22·39, 21·46; whilst ruthenium, rhodium, and palladium are represented by the figures 11·40, 11·36, 11, the average densities of the heavy and light divisions thus being respectively 22·43 and 11·25.

But a more interesting and important classification is what I may designate as a first and second class series, from the more important view of their relative properties of stability. Thus platinum, palladium, and rhodium form the first or higher class, not being volatilizable in a state of oxide; iridium, osmium, and ruthenium forming the second or lower class, their oxides being more or less readily volatilized.

The oxide of iridium is affected at 700 to 800° C., and entirely decomposed at 1,000°, whilst osmic and hyporuthenic acids are volatilized at the low degree of 100°, the latter exploding at 108°. The chlorides of these metals can be sublimed at different temperatures (as also the protochloride of platinum).

I now propose to give a short description of the methods I have employed for preparing the pure platinum and iridium necessary for the manufacture of the alloy, which I call "iridio-platinum," and it is upon the distinguishing characteristics above-mentioned that my method of separation is chiefly founded.

Platinum.

The preparation of this metal to a state of purity is an operation of extreme delicacy. I commence by taking ordinary commercial platinum; I melt this with six times its weight of lead of ascertained purity, and, after granulation, dissolve slowly in nitric acid diluted in the proportion of 1 volume to 8 of distilled water. The more readily to ensure dissolution, it is well to place the granulated alloy in porcelain

baskets such as are used in the manufacture of chlorine gas for holding the oxide of manganese. When the first charge of acid is sufficiently saturated, a fresh quantity should be added until no more action is apparent; at this stage the greater part of the lead will have been dissolved out together with a portion of any copper, iron, palladium, or rhodium that may have been present. These metals are subsequently extracted from the mother-liquors, the nitrate of lead by crystallization, and the remaining metals by well-known methods.

The metallic residue now obtained will be found in the state of an amorphous black powder (a form most suitable for further treatment), consisting of platinum, lead, and small proportions of the other metals originally present—the iridium existing as a brilliant crystalline substance insoluble in nitric acid. After digesting this compound in weak aqua regia, an immediate dissolution takes place of the platinum and lead, leaving the iridium still impure, but effecting a complete separation of the platinum.

To the chloride of platinum and lead after evaporation is added sufficient sulphuric acid to effect the precipitation of the whole of the lead as a sulphate, and the chloride of platinum after dissolution in distilled water is treated with an excess of chloride of ammonium and sodium, the excess being necessary in order that the precipitated yellow double salt may remain in a saturated solution of the precipitant. The whole is then heated to about 80°, and allowed to stand for some days; the ammonio-chloride of platinum will settle down as a firm deposit at the bottom of the vessel, whilst if any rhodium, as is generally the case, is present, the surface liquor will be coloured a rose tint, occasioned by a combination of the salts of the two metals.

The precipitate must be repeatedly washed with a saturated solution of chloride of ammonium and subsequently with distilled water charged with pure hydrochloric acid. This is necessary for its purification. The small quantity of the double salt which will be taken up and held in solution is of course recovered afterwards. Rhodium may still exist in the washed precipitate, which must therefore not be reduced to the metallic state until its separation is completed, and this is best effected by mixing with the dried compound, salts of chloro-platinate and chloro-rhodiate of ammonia, bi-sulphate of potash with a small proportion of bi-sulphate of ammonia, and subjecting to a gradual heat brought by degrees up to a dull red in a platinum capsule, over which is placed an inverted glass funnel. The platinum is thus slowly reduced to a black spongy porous condition freed from water, nitrogen, sulphate of ammonia, and hydrochloric acid, the rhodium remaining in a soluble state as bi-sulphate of rhodium and potash, which can be dissolved out completely by digesting in boiling distilled water; a small quantity of platinum will have been taken up in a state of sulphate, but is regained by heating the residue (obtained

on evaporation) to redness, at which heat it is reduced to the metallic condition, the rhodium salt remaining undecomposed.

By the method above described the platinum is freed not only from rhodium, but from all other metals with which it may have been contaminated, and is brought to a state of absolute purity, of the density 21·46, the highest degree obtainable.

Iridium.

In the preparation of this metal when intended to be used for the manufacture of iridio-platinum alloy, I have arrived at freeing it to the utmost possible extent from all its associate metals, except platinum, disregarding the presence of the latter; the proportion of which, once determined, would only form matter of calculation in the final operation of mixing my alloy.

In practice, the purest iridium which can be obtained from its ordinary solution (deprived of osmium by long boiling in aqua regia and precipitated by chloride of ammonium) will almost invariably contain traces of platinum, rhodium, ruthenium, and iron.

I fuse such iridium in a fine state of division with ten times its weight of lead, keeping it in a molten state for some hours, dissolve out the lead with nitric acid, subject the residue to a prolonged digestion in aqua regia, and obtain a crystalline mass composed of iridium, rhodium, ruthenium, and iron, in a condition suitable for my further treatment. By fusion at a high temperature with an admixture of bi-sulphate of potash, the rhodium is almost entirely removed, any remaining trace being taken up together with the iron in a later operation. The iridium so far prepared is melted with ten times its weight of dry caustic potash, and three times its weight of nitre, in a gold pan or crucible; the process being prolonged for a considerable time to effect the complete transformation of the material into iridiate and ruthenate of potash, and the oxidation of the iron; when cold, the mixture is treated with cold distilled water. The iridiate of potash of a blue tinge will remain as a deposit almost insoluble in water, more especially if slightly alkaline, and also the oxide of iron.

This precipitate must be well washed with water charged with a little potash and hypochlorite of soda until the washings are no longer coloured, and then several times with distilled water.

The blue powder is then mixed with water strongly charged with hypochlorite of soda, and allowed to remain for a time cold, then warmed in a distilling vessel, and finally brought up to boiling point until the distillate no longer colours red, weak alcohol acidulated with hydrochloric acid.

The residue is again heated with nitre and potash water charged with hypochlorite of soda and chlorine, until the last trace of ruthenium has disappeared.

Further, to carry out the purification, the blue powder (oxide of iridium) is re-dissolved in aqua regia, evaporated to dryness, re-dissolved in water, and filtered.

The dark-coloured solution thus obtained is slowly poured into a concentrated solution of soda and mixed with hypochlorite of soda, and should remain as a clear solution without any perceptible precipitate, and subjected in a distilling apparatus to a stream of chlorine gas, should not show a trace of ruthenium when hydrochloric acid and alcohol are introduced into the receiver. In this operation the chlorine precipitates the greater part of the iridium in a state of blue oxide, which after being collected, washed, and dried, is placed in a porcelain or glass tube, and subjected to the combined action of oxide of carbon and carbonic acid obtained by means of a mixture of oxalic with sulphuric acid gently heated.

The oxide of iridium is reduced by the action of the gas leaving the oxide of iron intact, the mass is then heated to redness with bi-sulphate of potash (which will take up the iron and any remaining trace of rhodium) and after subjecting it to many washings with distilled water, the residue is washed with chlorine water to remove any trace of gold, and finally with hydrofluoric acid, in order to take out any silica which might have been accidentally introduced with the alkalies employed or have come off the vessels used.

The iridium after calcination at a strong heat in a charcoal crucible, is melted into an ingot, and after being broken up and boiled in hydrochloric acid, to remove any possible trace of iron adhering to it through the abrasion in breaking up, should possess if perfectly pure a density of 22·39; but, as iridium prepared even with the utmost care will still contain minute though almost inappreciable traces of oxygen, ruthenium, rhodium, and possibly iron, the highest density I have yet attained is 22·38.

Alloy of Iridio Platinum.

This compound metal possesses physical properties of great value, forming a beautiful example of the effect of a careful combination of the opposite characteristics of its component parts. Thus, the extreme softness and expansiveness of pure platinum and the brittleness and excessive hardness of pure iridium, produce, by combination in judicious proportions, a perfect and homogeneous alloy, possessing the necessary mean of these properties to render it suitable for many important purposes, amongst others that of the special object to be attained to meet the requirements for an unalterable standard metal, for which it is peculiarly adapted.

In the manufacture of the prototype metres and the geodesique rules (each 4 metres in length) ordered from my firm by the Comité Internationale des Poids et Mesures, the Association Géodésique In-

ternationale, and the French Minister of War, I proceeded in the following manner with the platinum and iridium prepared as described above.

Operating upon a charge of 450 ounces of platinum and 55 ounces of iridium, I commenced by melting these metals together and casting into an ingot of suitable shape, which I then cut into small pieces with hydraulic machinery. After re-melting and retaining in a molten condition under a powerful blast of oxygen and common gas for a considerable time, I re-cast and forged at an intense white heat under a steam hammer, the highly polished surfaces of which were cleaned and polished after each series of blows—when sufficiently reduced it was passed through bright polished steel rollers, cut into narrow strips, and again slowly melted in a properly shaped mould, in which it was allowed to cool. I thus obtained a mass of suitable shape for forging, perfectly solid, homogeneous, free from fissures or air-holes, and with a bright and clean surface at bottom and sides as at top. At the first forging a bar was obtained 35 centims. long, 7·5 wide, 2·5 thick, which weighed—

In air	15·105 grms.
In water at 70° F.	14·405 „
Showing a density at zero of	21·522

A third of the bar was cut off and the larger portion again forged to a length of 95 centims., width 2·5, thickness 2·0, which weighed—

In air	10·814 grms.
In water at 60° F.	10·315 „
Showing a density at zero of	21·648

This was then passed through highly polished rolls until of a length of 4,010 centims. 21 millims. in width, and 5 millims. thick, to which a perfectly rectangular form was subsequently given by drawing it through a series of plates, and thus prepared the rule was in a condition to receive the beautiful polish of which this alloy is susceptible.

After passing it through each hole the metal was annealed by means of a jet of gas and oxygen to a heat just below melting point, and each time throughout after forging, rolling, and drawing was exposed to the action of melted borax, and boiled in concentrated hydrochloric acid to remove any possible trace of adherent iron or other impurity.

A piece cut from the end and presented to the French Academy of Science gave the following results:—

Weight in air	116·898 grms.
„ water	111·469 „
Showing a density of..	21·516

thus proving that the necessary processes of annealing at a high temperature had caused it sensibly to resume its original density.

The analysis gave—

	1.	2.
Platinum	89·40	89·42
Iridium	10·16	10·22
Rhodium	0·18	0·16
Ruthenium	0·10	0·10
Iron	0·06	0·06
	99·90	99·96

From which is deduced:—

	Proportion.	Density at zero.	Volume.
Iridio-platinum, at 10 per cent.	99·33	21·575	4·603
Iridium, in excess	0·23	22·380	0·010
Rhodium	0·18	12·000	0·015
Ruthenium	0·10	12·261	0·008
Iron........................	0·06	7·700	0·008
	99·90		4·644

Density at zero, calculated after No. 1 analysis.... 21·510
Density at zero, calculated after No. 2 „ ....21·515

thus coinciding perfectly with the practical results obtained.

Messrs. Leon Brunner Brothers, of Paris, who had submitted this material of the géodésique rule to a great number of mechanical experiments, communicated the result of their observations to M. H. Sainte-Claire Deville, thus:—

"Paris, 27 Août, 1878.

"Monsieur,

"La division de la règle géodésique, que nous faisons pour l'Association Géodésique Internationale, est terminée depuis quelques jours.

"Nous avons pensé que vous ne seriez pas mécontent d'apprendre que cette operation a parfaitement réussi, et que c'est au métal que nous attribuons la facilité avec laquelle nous avons pu l'exécuter.

"Le platine iridié de M. Matthey est incontestablement supérieur au platine ordinaire, pour la confection des règles divisées. Il est exempt de ces pailles qu'on rencontre toujours dans ce dernier, et se laisse polir au charbon. On peut, sans danger, enlever les rébarbes des traits et les conserver très beaux. Le platine ordinaire ne peut-être poli qu'au papier à émeré, et l'on est toujours exposé à gâter la division quand on procéde à l'ébarbage. C'est là un inconvénient très-grave.

"Nous ne pouvons que vous remercier, Monsieur, d'avoir mis à notre disposition un metal qui modifie singulièrement les difficultés

qu'on rencontre dans la fabrication d'une règle géodésique, et nous vous prions de recevoir l'assurance de nos sentiments les plus distingues.

"Brunner Freres."

In the year 1876 the suggestion was made to supersede the rectangular form by a tubular one, and I was requested to produce one of the following dimensions: Length, 1,002 centims.; exterior diameter, 37 millims.; interior diameter, 35 millims.; with rounded ends, one having an extension of small tube 4 millims. exterior diameter, 2 millms. interior diameter, 40 millms. long, which I did by the system of tube making with autogenous joints adopted by me with excellent results for the last 20 years, employing for the purpose an alloy prepared as above described. These proved to be so satisfactory that I have since made others, both round and square, of various dimensions, as lately shown at the Paris Exhibition.

Iridio-platinum alloy has now been proved to possess the following among many advantages for standard rules and weights:—

It is almost indestructible, has extreme rigidity, especially in the tube form, and a most beautifully polished surface can be obtained upon it; its coefficient of elasticity is very great, whilst for standard weights its high density is a valuable quality, and for these I should indeed recommend an alloy of not less than 20 per cent. of iridium. I lately made at the request of M. H. Sainte-Claire Deville a cylinder 40 millims. by 40 millims. of such an alloy, which showed by analysis the following proportions):—

Platinum	80·6600
Iridium	19·0786
Rhodium	·1220
Ruthenium	·0460
Iron	·0980
	100·0046

and gave the density of 21·614.

With such a high density its coefficient of elasticity is 22·200000, one of the highest known, whilst its malleability and ductility are almost without limit.

The volume of the kilogram thus prepared is only 46·266 cub. cemtims., it displaces 2·267 cub. centims. less than the kilogram of the archives of France, and on this account, as on many others, is of course preferable.

The results I have arrived at in preparing alloys of higher grades, viz., 25—30—40 and 50 per cent. of iridium, are as follows:—

The alloy of 20 per cent. iridium is, as I have stated already, malleable and ductile.

25 per cent. can only with great difficulty and waste be worked

into sheet and wire when heated at low temperature. 30 per cent. and 40 per cent. with great difficulty only at a temperature little less than melting point, being brittle when cold, but with a grain of great beauty and fineness.

50 per cent. I have as yet failed to work up into forms other than castings beyond what I can effect by pressure when in a semi-fused condition.

The general results of my work on this alloy would lead me, therefore, to make the following recommendations.

For the manufacture of standard rules to use an alloy of not less than 85 per cent. platinum and 15 per cent. iridium, adopting the tubular form.

For the standard weights to use an alloy of not less than 80 per per cent. platinum and 20 per cent. iridium, adopting the form now generally made.

Finally, following the expression of the great French chemist, M. Dumas, I hope by these labours "d'avoir enriché l'outillage scientifique d'un alliage doué des propriétés précieuse."

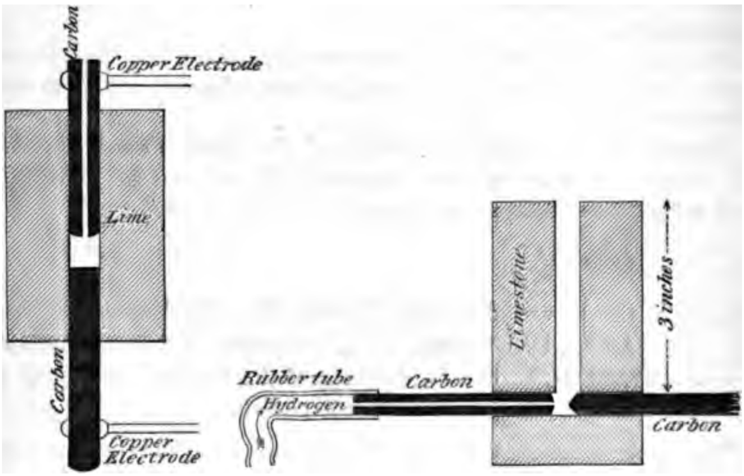
III. "On the Reversal of the Lines of Metallic Vapours." No. VI. By G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received March 27, 1879.

The experiments described in the following communication were made with the electric arc, and in lime crucibles,* or in crucibles of a highly calcareous sandstone, kindly supplied to us by Messrs. Johnson, Matthey, and Co., as described in our fourth communication on this subject; but for some of them we used, instead of a galvanic battery, a magneto-electric machine producing a much more powerful current and a much longer arc. The experiments with this machine were made, through the kindness of Dr. Tyndall, at the Royal Institution, and we are indebted to Messrs. Siemens both for the working of the machine and for sparing to us the services of a skilled engineer, in

* In our first paper on this subject, communicated in February, 1878, when referring to the experiments of Lockyer and Roberts ("Proc. Roy. Soc.," xxiii), we mentioned that they employed the combined action of a charcoal furnace and an oxyhydrogen blowpipe, but omitted to mention that they used a lime chamber after the model of Stas. Referring to fig. 1 in our communication of February 12, 1879, where the use of an oxyhydrogen blowpipe in a lime block is represented, we disclaim any novelty in the use of lime; the difference between our experiments and theirs consisting in this, that we use the continuous spectrum from the hot walls of our crucible, instead of an external independent source of light, as a background against which the absorbent action of the vapours is seen, in the same way as we had previously used iron tubes, and now use the electric arc.

the person of Mr. Oscar Doermer, whose assistance was most valuable. We wish to express our thanks to all these gentlemen for the facilities they so readily granted to us.

The results obtained with the powerful current from the magneto-electric machine did not differ at all in kind from those obtained with the battery, and much less in degree than we had expected. We had really but one day's work with this machine, which we can only regard as a preliminary trial of it, and, in the meantime, until we have the opportunity of a longer series of experiments with it, we communicate the results obtained to the Royal Society.



In some cases we have introduced a current of hydrogen, or of coal-gas, into the crucibles by means of a small lateral opening, or by a perforation through one of the carbon electrodes; sometimes the perforated carbon was placed vertically, and we examined the light through the perforation (see diagrams). When no such current of gas is introduced, there is frequently a flame of carbonic oxide burning at the mouth of the tube, but the current of hydrogen produces very marked effects. As a rule, it increases the brilliance of the continuous spectrum, and diminishes relatively the apparent intensity of the bright lines, or makes them altogether disappear with the exception of the carbon lines. When this last is the case, the reversed lines are seen simply as black lines on a continuous background. The calcium line with wave-length 4226 is always seen under these circumstances as a more or less broad black band on a continuous background, and when the temperature of the crucible has risen sufficiently, the lines with wave-lengths 4434 and 4454, and next that with wave-length 4425, appear as simple black lines. So too do the *blue and* red lines of lithium, and the barium line of wave-length 5535,

appear steadily as sharp black lines, when no trace of the other lines of these metals, either dark or bright, can be detected. Dark bands also frequently appear, with ill-defined edges, in the positions of the well-known bright green and orange bands of lime.

In the case of sodium, using the chloride, we have repeatedly reversed the pair of lines (5687, 5681) next more refrangible than the D group. In every case the less refrangible of the two was the first to be seen reversed, and was the more strongly reversed, as has also been observed by Mr. Lockyer. But our observations on this pair of lines differ from his in so far as he says that "the double green line of sodium shows scarcely any trace of absorption when the lines are visible," while we have repeatedly seen the reversal as dark lines appearing on the expanded bright lines; a second pair of faint bright lines, like ghosts of the first, usually coming out at the same time on the more refrangible side.

Using potassium carbonate, besides the violet and red lines which had been reversed before, we saw the group, wave-lengths 5831, 5802, and 5782, all reversed, the middle line of the three being the first to show reversal. Also the lines wave-lengths 6913, 6946, well reversed, the less refrangible remaining reversed the longer. Also the group, wave-lengths 5353, 5338, 5319 reversed, the most refrangible not being reversed until after the others. Also the line wave-length 5112 reversed, while two other lines of this group, wave-lengths 5095 and 5081, were not seen reversed.

Using lithium chloride, not only were the red and blue lines, as usual, easily reversed, and the orange line well reversed for a long time, but also the green line was distinctly reversed; the violet line still unreversed, though broad and expanded. Had this green line belonged to cæsium, the two blue lines of that metal which are so easily reversed could not have failed to appear; but there was no trace of them.

In the case of rubidium, we have seen the less refrangible of the red lines well reversed as a black line on a continuous background, but it is not easy to get, even from the arc in one of our crucibles, sufficient light in the low red to show the reversal of the extreme ray of this metal.

With charred barium tartrate, and also with baryta and aluminium together, we have obtained the reversal of the line with wave-length 6496, besides the reversals previously described. The less refrangible line, wave-length 6677, was not reversed.

With charred strontium tartrate, the lines with wave-lengths 4812, 4831, and 4873, were reversed, and by the addition of aluminium, the line wave-length 4962 was reversed for a long time, and lines wave-lengths 4895, 4868, about, were also reversed.

On putting calcium chloride into the crucible, the line wave-length

4302 was reversed, this being the only one of the well-marked group to which it belongs which appeared reversed. On another occasion, when charred strontium tartrate was used, the line wave-length 4877 was seen reversed, as well as the strontium line near it. Also the lines wave-lengths 6161, 6121, have again been seen momentarily reversed.

With magnesium, no new reversals of the lines of the metal have been observed by us; but when a stream of hydrogen or of coal-gas was led into the crucible, the line wave-length 5210, previously seen by us in iron tubes, and ascribed by us to a combination of magnesium with hydrogen, was regularly seen, usually as a dark line, sometimes with a tail of fine dark lines on the more refrangible side similar to the tail of bright lines seen in the sparks taken in hydrogen between magnesium points. Sometimes, however, this line (5210) was seen bright. It always disappeared when the gas was discontinued, and appeared again sharply on re-admitting the hydrogen. These effects were, however, only well-defined in crucibles having a height of at least 3 inches above the arc.

On putting a fragment of metallic gallium into a crucible, the less refrangible line, wave-length 4170, came out bright, and soon a dark line appeared in the middle of it. The other line, wave-length 4031, showed the same effect, but less strongly.

In the cases of cadmium and copper, though we have made no thorough examination of them, we can corroborate the results arrived at by Cornu. We noticed particularly the disappearance in the arc of the cadmium lines, with wave-lengths 5377 and 5336.

On the addition of aluminium to either copper or silver in our lime crucibles, we noticed that the copper or silver lines which had been previously predominant, almost faded away, while the calcium lines came out instead with marked brilliancy. In no case could we detect the red lines of aluminium in the arc.

With a view to re-introduce into the arc the magnesium line wave-length 4481, we tried the action of an induction spark in a lime crucible simultaneously with the arc, but without success; for the conducting power of the hot walls of the crucible, and the highly expanded gases within it, caused the resistance to be so much diminished, that the spark passed as in a highly rarefied medium. In order to succeed with this experiment, it seems plain that it must be made in an apparatus which will allow of its being performed under a pressure of several atmospheres.

Reviewing the series of reversals which we have observed, we may remark that in many cases the least refrangible of two lines near together is the most easily reversed, as has been previously remarked by Cornu. Thus, in the case of barium (though there is no very distinct grouping of the lines of that metal) taking the rays in order, we have

the line wave-length 5535 readily reversed, while that with wave-length 5518 is less easily reversed; the line wave-length 4933 is comparatively easily reversed, whereas that with wave-length 4899 has not been reversed by us. On the other hand, the line wave-length 4553 has been reversed, but not the line wave-length 4524. In the case of strontium, the lines wave-length 4831 and 4812 have been reversed, but not the line wave-length 4784, and the two lines wave-length 4741 and 4721 remain both unreversed. In the group of five lines of calcium, wave-length 4318 to 4282, it is only the middle line wave-length 4302 which has been reversed. Of the potassium groups of lines wave-length 5831 and 5782, 5802, 5782 are reversed, the line wave-length 5811 has not been reversed, and of the others the line wave-length 5802 is the first to appear reversed. It is worthy of remark that the first of these lines is faint and the last is the brightest of the group. The group wave-length 5355, 5336, 5319 have been all reversed, but the last of the three (5319) was the most difficult to reverse: it is also the feeblest of the group. In the more refrangible group, wave-length 5112, 5095, 5081, the least refrangible is the only one reversed.

Making a general summation of our results respecting the alkaline earth metals, potassium, and sodium, and having regard only to the most characteristic rays, which for barium we reckon as 21, for strontium 34, for calcium 37, for potassium 31, and for sodium 12, the reversals in our experiments number respectively 6, 10, 11, 13, and 4. That is in the case of the alkaline earth metals about one-third, and these chiefly in the more refrangible third of the visible spectrum, the characteristic rays remaining unreversed in the more refrangible part of the spectrum being respectively 2, 5, and 4. In the case of potassium we reversed two in the upper third; all the rest in the least refrangible third. These experiments relate to mixtures of salts of these metals combined with the action of reducing agents. In a future communication we will contrast these results with those of the isolated metals, calcium, strontium, and barium.

IV. "Note on the unknown Chromospheric Substance of Young." By G. D. LIVEING, M.A., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received March 27, 1879.

In the preliminary catalogue of the bright lines in the spectrum of the chromosphere published by Young in 1861, he calls special attention to the lines numbered 1 and 82 in the catalogue, remarking that "they are very persistently present, though faint, and can be distinctly seen in the spectroscope to belong to the chromosphere, as such, not being due, like most of the other lines, to the exceptional elevation

of matter to heights where it does not properly belong. It would seem very probable that both these lines are due to the same substance which causes the D_3 line."

Again, in a letter to "Nature," June, 1872, Young says, "I confess I am sorry that the spectrum of iron shows a bright line coincident with 1474 (K); for, all things considered, I cannot think that iron vapour has anything to do with this line in the spectrum of the corona, and the coincidence has only served to mislead. But there are in the spectrum many cases of lines belonging to the spectra of different metals coinciding, if not absolutely, yet so closely, that no existing spectroscope can separate them, and I am disposed to believe that the close coincidence is not accidental, but probably points to some physical relationship, some similarity of molecular constitution perhaps, between the metals concerned. . . . So, in the case of the green coronal matter, is it not likely that though not iron it may turn out to bear some important relation to that metal?" In 1876 he proves that the coronal line 1474 is not actually coincident with the line of iron.

In the catalogue of bright lines observed by Young at Sherman in the Rocky Mountains, to which we have directed special attention in one of our previous communications, it appears that the above-mentioned lines 1 and 82, along with D_3, were as persistently present as hydrogen, the only other line approaching them in frequency of occurrence being the green coronal line 1474 of Kirchhoff, which was present on 90 occasions out of 100. It has occurred to us that these four lines may belong to the same substance. An analogy in the ratio of the wave-lengths of certain groups of lines occurring in different metals has been already pointed out by Stoney, Mascart, Salet, Boisbaudran, and Cornu; and without any special reductions, or claims to an exact ratio in whole numbers, the following analogies are worthy of note:—

Hydrogen.	Lithium.	Magnesium.	Chromospheric substance.
Wave-length:—			
(1) 6563·9	(1) 6706	(1) 5183	(1) 7055 *
(2) 4862·1	(2) 6102	(2) 3837·8	(2) 5874·9
(3) 4340	(3) 4970	(3) 3335	(3) 5315·9
(4) 4102·4	(4) 4604		(4) 4471·2
	(5) 4130		
Ratio of wave-length of—			
(1) (2) & (4)	(1) (3) & (5)	(1) (2) (3)	(1) (3) & (4)
$\frac{1}{20}$ $\frac{1}{27}$ $\frac{1}{32}$	$\frac{1}{20}$ $\frac{1}{26\cdot9}$ $\frac{1}{31\cdot6}$	$\frac{1}{20}$ $\frac{1}{27}$ $\frac{1}{31\cdot1}$	$\frac{1}{20}$ $\frac{1}{26\cdot5}$ & $\frac{1}{31\cdot6}$

* This wave-length is not so accurately known as the other rays belonging to the chromosphere.

The ratio of the wave-lengths of F to G of hydrogen ((2) to (3) in the table above) is nearly identical with the ratio of D_3 to the coronal green line ((2) to (3) in table above).

This near coincidence in the ratios of certain lines of hydrogen, lithium, and magnesium, substances belonging to the same type, combined with a similar ratio in the wave-lengths of the nearly equally persistent lines of the chromosphere, greatly strengthens the probability of the assumption that these lines belong to one substance.

The fact that the two less refrangible rays have no representative in the Fraunhofer lines, is by no means opposed to their belonging to one substance, since we know that aluminium behaves in a similar way in the atmosphere of the sun; and in the total eclipse of 1875 the hydrogen line *h* was not visible in the chromosphere, that is, we suppose, was on the limit between brightness and reversal; and during the late eclipse the two most refrangible rays of hydrogen were not detected from the same cause.

Until our knowledge of the order of reversibility of lines belonging to different types of metals has been extended, it would be rash to infer the group of metals to which it belongs, or its probable molecular weight.

V. "Contributions to Molecular Physics in High Vacua." By WILLIAM CROOKES, F.R.S. Received March 27, 1879.

(Abstract.)

This paper is a continuation of one "On the Illumination of Lines of Molecular Pressure, and the Trajectory of Molecules," which was read before the Royal Society on the 5th of December last. The author has further examined the action of the molecular rays electrically projected from the negative pole in very highly exhausted tubes, and finds that the green phosphorescence of the glass (by means of which the presence of the molecular rays is manifested) does not take place close to the negative pole. Within the dark space there is absolutely no phosphorescence; at very high exhaustions the luminous boundary of the dark space disappears, and now the phosphorescence extends all over the sensitive surface. Assuming that the phosphorescence is due either directly or indirectly to the impact of the molecules on the phosphorescent surface, it is reasonable to suppose that a certain velocity is required to produce the effect. The author adduces arguments to show that within the dark space, at a moderate exhaustion, the velocity does not accumulate to a sufficient extent to produce phosphorescence, but at higher exhaustions the mean free path is long enough to allow the molecules to get up sufficient speed

to excite phosphorescence. At a very high exhaustion there are fewer collisions, and the initial speed of the molecules close to the negative pole not being thereby reduced, phosphorescence takes place close to the pole.

Experiments are described in which a pole folded into corrugations is used at one end of a tube, the pole at the other end being flat set obliquely to the axis of the tube, and having a plate of mica in front pierced with a hole opposite the centre of the pole. The questions which this apparatus was designed to answer are:—(1.) Will there be two sets of molecular projections from the corrugated pole when made negative, one perpendicular to each facet, or will the projection be perpendicular to the electrode as a whole, *i.e.*, along the axis of the tube? (2.) Will the molecular rays from the oblique flat pole, when this is made negative, issue through the aperture of the screen along the axis of the tube, *i.e.*, direct to the positive pole, or will they leave the pole normal to the surface and strike the glass on its side? With the corrugated pole experiment shows that at high exhaustions molecular rays are projected from each facet to the inner surface of the tube, where they excite phosphorescence, and form portions of ellipses by the intersection of the planes of molecular rays with the cylindrical tube. When the oblique flat pole is made negative, a stream of molecules shoots from it nearly normal to its surface, and those which pass through the hole in the plate of mica strike the side of the tube, forming an oval patch of a green colour.

The oval patch in this apparatus happens to fall on a portion of the glass which has previously had its phosphorescence excited by the molecular discharge from the other corrugated pole. The phosphorescence from this pole is always more intense than that from the flat pole, and the glass, after having been excited by the energetic bombardment, ceases to respond readily to the more feeble excitement from the flat pole. The effect, therefore, is, that when the oval spot appears, it has a dark band across it where the phosphorescence from the other pole had been taking place. The glass recovers its phosphorescent power to some extent after rest.

In this apparatus a shifting of the line of molecular discharge is noticed. If the coil is stopped and then set going repeatedly, always keeping the oblique pole negative, the spot of green light occurs on the glass at the spot where it should come supposing the discharge were normal to the surface of the pole. But if once the flat pole is made positive, the next time it is made negative the spot of light appears nearer the axis of the tube, and instantly shifts to its normal position, where it remains so long as its pole is made negative. There seems no limit to the number of times this experiment can be repeated.

A suggestion having been made by Professor Stokes that a third, idle, pole should be introduced between the negative and positive elec-

trodes, experiments are described with an apparatus constructed accordingly. The potential of the idle poles (of which there are two) at low exhaustions is very feebly positive; as the exhaustion gets better the positive potential increases, and at a vacuum so good as to be almost non-conducting, the positive potential of the idle poles is at its greatest. The result is that an idle pole in the direct line of fire between the positive and negative poles, and consequently receiving the full impact of the molecules driven from the negative pole, has a strong positive potential.

It is found that when the shadow of an idle pole is projected on a phosphorescent screen, the trajectory of the molecules suffers deflection when the idle pole is suddenly uninsulated by connecting it with earth. The same result is produced by connecting the idle pole with the negative wire through a very high resistance, such as a piece of wet string, instead of connecting it with earth. A tube, which has already been described in a paper read before the Royal Society on December 5th last, is used to illustrate this deflection. The shadow of an aluminium star is projected on a phosphorescent screen. So long as the metal star is insulated the shadow remains sharp, but on uninsulating the star by connecting it with an earth wire the shadow widens out, forming a tolerably well-defined penumbra outside the original shadow, which can still be seen unchanged in size and intensity. On removing the earth connexion the penumbra disappears, the umbra remaining as before.

It is also found that the shadow of the star is sharply projected when it is made the positive pole, the negative pole remaining unchanged.

These experiments are explained by the results just mentioned, that the idle pole, the shadow of which is cast by the negative pole, has strong positive potential. The stream of molecules must be assumed to have negative potential; when they actually strike the idle pole they are arrested, but those which graze the edge are attracted inwards by the positive potential and form the umbra. When the idle pole is connected with earth, its potential would become zero were the discharge to cease; but inasmuch as a constant supply of positive electricity is kept up from the passage of the current, we must assume that the potential of the idle pole is still sufficient to more than neutralize the negative charge which the impinging molecules would give it. The effect, therefore, of alternately uninsulating and insulating the idle pole is to vary its positive potential between considerable limits, and consequently its attractive action on the negative molecules which graze its edge. The result is a wide or a narrow shadow, according to circumstances.

After a definite shadow is produced, it is found that increasing the exhaustion makes very little change in the umbra, but it causes the

penumbra' to increase greatly in size. Experiments recorded in the paper already quoted have proved that the velocity of the molecules is greater as the vacuum gets higher, and consequently the trajectory of the molecules under deflecting action, whether of a magnet or of an insulated idle pole, is flatter at high than at low vacua.

An experiment is next described, having for its object to ascertain whether two parallel molecular rays from two adjacent negative poles attract or repel each other. It is considered that if the stream carries an electric current, attraction should ensue, but if they are simply streams of similarly electrified bodies, the result would be repulsion. Experiment proves that the latter alternative happens, lateral repulsion taking place between two streams moving in the same direction.

Many experiments are given to illustrate the law of action of magnets on the molecular stream, but the results are of too complicated a character to bear condensation without the diagrams accompanying the original paper.

The molecular stream is sufficiently sensitive to show appreciable deflection by the magnetism of the earth.

The author, after numerous experiments, has succeeded in obtaining continuous rotation of the molecular stream under the influence of a magnet, analogous to the well-known rotation at lower exhaustions. Comparative experiments are given with a "high vacuum" tube, where no luminous gas is visible, but only green phosphorescence on the surface of the glass, and a "low vacuum" tube, in which the induction spark passes in the form of a luminous band of light joining the two poles. These two tubes are mounted over similar electromagnets, the direction of discharge being in a line with the axis of the magnet. Numerous experiments, the details of which are given in the paper, show that the law is not the same at high as at low exhaustions. At high exhaustions the magnet causes the molecular rays to rotate in the same direction, whether they are coming towards the magnet or going from it; the direction of rotation being entirely governed by the magnetic pole presented to the stream. The north pole rotates the molecular discharge in a direct* sense, independent of the direction in which the induction current passes. The direction of rotation impressed on the molecules by a magnetic pole is opposite to the direction of the electric current circulating round the magnet. These results offer an additional proof that the stream of molecules driven from the negative pole in high vacua do not carry an electric current in the ordinary sense of the term.

The author, after giving details of experiments in which platinum and glass are fused in the focus of converging molecular rays projected from a concave pole, describes observations with the spectroscope,

* Like the hands of a watch.

which show that glass obstinately retains at even a red heat a compound of hydrogen—probably water—which is only driven completely off by actual fusion.

The permanent deadening of the phosphorescence of glass is shown by projecting the shadow of a metal cross on the end of a bulb for a considerable time. On suddenly removing the cross, its image remains visible, bright upon a dark ground.

One of the most striking of the phenomena attending this research is the remarkable power which the molecular rays in a high vacuum have of causing phosphorescence in bodies on which they fall. Substances known to be phosphorescent under ordinary circumstances shine with great splendour when subjected to the negative discharge in a high vacuum. Thus Becquerel's luminous sulphide of calcium has been found invaluable in this research for the preparation of phosphorescent screens whereon to trace the paths and trajectories of the molecules. It shines with a bright blue-violet light, and when on a surface of several square inches is sufficient to faintly light a room.

The only body which the author has yet met with which surpasses the luminous sulphides, both in brilliancy and variety of colour, is the diamond. Most diamonds from South Africa phosphoresce with a blue light. Diamonds from other localities shine with different colours, such as bright blue, apricot, pale blue, red, yellowish-green, orange, and pale green. One very beautiful diamond in the author's collection gives almost as much light as a candle when phosphorescing in a good vacuum.

Next to the diamond alumina and its compounds are the most strikingly phosphorescent. The ruby glows with a rich full red, and it is of little consequence what degree of colour the stone possesses naturally, the colour of the phosphorescence is nearly the same in all cases; chemically prepared and strongly ignited alumina phosphoresces with as rich a red glow as the ruby. The phosphorescent glow does not therefore depend on the colouring matter. E. Becquerel* has shown by experiments with his phosphoroscope, that alumina and many of its compounds phosphoresce of a red colour after insolation.

Nothing can be more beautiful than the effect presented by a mass of rough rubies when glowing in a vacuum; they shine as if they were red hot, and the illumination effect is almost equal to that of the diamond under similar circumstances.

Masses of artificial ruby in crystals, prepared by M. Ch. Feil, behave in the vacuum like the natural ruby.

In the spectroscope the alumina glow shows one intense and sharp red line less refrangible than the line B, and a faint continuous spectrum ending at about B. The wave-length of the red line is 6895.

* "Annales de Chimie et de Physique," 3rd series, vol. lvii, p. 50.

The paper concludes with some notes by Professor Maskelyne, on the connexion between molecular phosphorescence and crystalline structure.

The crystals experimented on have been the diamond, emerald, beryl, sapphire, ruby, quartz, phenakite, tinstone, hyacinth (zircon), tourmaline, andalusite, enstatite, minerals of the augite class, apatite, topaz, chrysoberyl, peridot, garnet, and boracite. Of these, the only crystals which give out light are diamond, ruby, emerald, sapphire, tinstone, and hyacinth. The light from emerald is crimson, and is polarised, apparently completely, in a plane perpendicular to the axis. Sapphire gives out a bluish-grey and a red light polarised in a plane perpendicular to the axis. The ruby light exhibits no marked distinction in the plane of its polarisation.

Among positive crystals tinstone glows with a fine yellow light, polarised in a plane parallel to the axis of the crystal. So far the experiments accord with the quicker vibrations being those called into play, and therefore in a negative crystal the extraordinary, and in a positive crystal the ordinary, is the ray evoked. Hyacinth, however, introduces a new phenomenon, being dichroic, the colours, in three different crystals, being pale pink and lavender—blue, pale blue and deep violet, and yellow and deep violet-blue, polarised in opposite planes.

The only conclusion arrived at is, that the rays, whose direction of vibration corresponds to the direction of maximum optical elasticity in the crystal, are always originated where any light is given out. As yet, however, the induction on which so remarkable a principle is suggested, cannot be considered sufficiently extended to justify that principle being accepted as other than probable.

VI. "Note on a Direct Vision Spectroscope after Thollon's Plan, adapted to Laboratory use, and capable of giving exact Measurements." By G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received April 3, 1879.

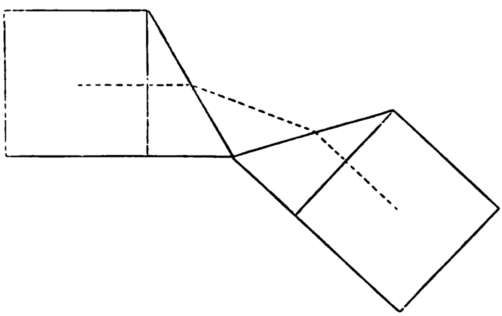
Having seen in the "Journal de Physique" for May, 1878, the account of M. Thollon's ingenious direct vision spectroscope, it occurred to us that by a little modification we could adapt his plan so as to produce an instrument well fitted for the work in which we were engaged, combining the advantage of excellent definition, which his plan secures, with the means of getting exact measurements with the least possible chance of errors of adjustment or inequalities in the working of the automatic system. The principle consists in having two prisms only (half prisms as M. Thollon calls them), of which one *is fixed*, and receives the light from the collimator by a reflecting

prism and transmits it in a plane at right angles to the axis of the collimator to the second prism.

This second prisim is moveable about an axis parallel to its edge and to the axis of the telescope, and has a right angled reflecting prism attached to it, so that the light after traversing this prism twice passes the second time through the fixed prism and so by reflection into the telescope. The lever carrying the second prism with its reflecting prism is moved by a micrometer screw, by the head of which the movement of the prism is read.

We placed the design in the hands of Mr. Hilger, some time since, and we now exhibit the instrument to the Society.

In the last number of the "Journal de Physique," M. Thollon describes some modification of his instrument, but it does not seem that his modified plan is so well adapted to the ordinary use of a chemical laboratory as ours.



The accompanying diagram represents a section through the prisms at right angles to the axis of the collimator and telescope.

April 24, 1879.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The Right Hon. Richard Assheton Cross, Secretary of State for the Home Department, was admitted into the Society.

The following Papers were read:—

I. "On the Nature of the Fur on the Tongue." By HENRY TRENTHAM BUTLIN, F.R.C.S. Communicated by J. BURDON SANDERSON, F.R.S., Professor of Physiology in University College, London. Received March 26, 1879.

[PLATES 10—13.]

The fur on the tongue is generally stated to consist chiefly of epithelial cells, usually sodden and granular. But several observers have described fungi as existing in it, or in the buccal mucus. Robin, for instance, describes a form of *Leptothrix* (*L. Buccalis*) in the mouth, and particularly in and between the teeth. Kölliker mentions, as of constant occurrence, masses or dark-brown bodies (which had previously been described by Miquel and Neidhardt, as occasionally present) having a granular aspect, which he believed to be of the nature of a fungus, similar if not identical with the fungus affecting the teeth. Billroth speaks of finding in the white fur of himself and of several patients, exquisite palmelloidal forms of *Ascococcus* and *Glœcoccus* colonies.

The object of this paper is to show that schizomycetes form the essential constituent of the fur, and to explain, as far as possible, some of the laws which govern the formation of fur.

The tongue is kept clean by free movement and by being rubbed against the interior of the mouth, the gums, and the teeth; but fur almost always exists upon its surface, both in health and in disease. The fur is generally thickest in the morning before food is taken, and during illness, when the necessary cleansing is not properly performed. It occurs, too, most abundantly in the centre and back part of the tongue, covering a triangular area immediately in front of the circumvallate papillæ, for this part of the tongue is most difficult to keep clean. It occupies the papillary surface of the tongue, scarcely ever extending beyond it, and is, therefore, not found posterior to the circumvallate papillæ. It does not form a continuous layer unless it is exceedingly thick, but lies upon the tops of the filiform and some of the fungiform papillæ. In children the fungiform papillæ are usually quite free from fur, but in adults the difference between the fungiform and filiform papillæ is not nearly so well marked, and, with the exception of those situated near the apex of the tongue, the fungiform papillæ are frequently coated. Fur forms upon the filiform papillæ, because these papillæ are rough and possessed of longer or shorter epithelial processes, to which foreign matters cling readily, and from which it is very difficult to dislodge them. The fungiform papillæ, on the contrary, are usually smooth and rounded on the summit, and even when large are easily kept clean.

The accompanying tables refer to the constancy of the presence of fur, to its thickness in health, and to its relation to the papillæ.

Analysis of Cases examined.

On 68 healthy tongues—fur on all except one.

On 178 tongues of persons suffering from disease or accident—fur on all except two.

Table showing relation of fur to papillæ on 62 healthy tongues, with remarks on the age of the persons and the characters of the papillæ.

Position of Fur.	No. of cases.	Age of Patients.	Remarks on Papillæ.
On filiform papillæ only.	41	22 under 20 years of age.	
On filiform and some fungiform papillæ.	18	17 over 20 years of age—1 æt. 15.	In all cases fungiform papillæ small—in 14 cases difficult to distinguish.
Equally on filiform and fungiform papillæ.	3	All over 20 years....	Fungiform papillæ small or indistinguishable.
	62		

Table of thickness of the fur on 68 healthy tongues, with remarks on the papillæ and the nature of the tongue.

Quantity of Fur.	No. of cases.	Papillæ.	Condition of Tongue.
None................	1	Scarcely any........	Tongue very smooth and supple.
Very thin............	17	Scarcely any in 12...	Very smooth and supple in 12.
Thin	38		
Moderately thin.......	10	Large and distinct in 8.	
„ thick......	2	Large and distinct...	Infirm old people, æt. 80 and 95.
Thick................	0		
	68		

When thin the fur can only be scraped off with difficulty, and always brings with it numerous fragments of the hair-like processes which form the terminations of the filiform papillæ. But, when thicker, soft, and moist, it can be removed in considerable quantity with ease.

Microscopical examination of the results of such scraping gives, in nearly every instance, the same results.

1. *Débris* of food and bubbles of mucus and saliva.

2. Epithelium.

3. Masses which appear at first to consist of granular matter, but which are the glœa of certain forms of schizomycetes. When large and closely packed they are of yellow or yellowish-brown colour, but when smaller and more loosely held together are almost colourless. They are generally attached to portions of the hair-like processes which have come away with them, on account of the tenacity with which they adhere to the processes. Vertical sections of hardened tongues show the relation of these masses to the filiform papillæ better than mere scrapings of the surface of the tongue. The filiform papillæ, instead of exhibiting fine, clean, tapering processes, terminate in processes which are uneven, tuberculated, or beaded, and blunted at their ends, owing to the presence of these bodies. Around the masses float free fungi, often exhibiting very active movement. The relative proportion of the three constituents of fur varies under certain conditions. The quantity of *débris* of food and bubbles is much greater during or immediately after eating than during fasting, although there is no corresponding increase of the fur at such times. The epithelium is much more abundant in thin fur than in thick fur, its quantity depending rather upon the vigour with which the tongue is scraped than upon the amount of fur present. It can be obtained in just as great quantity where no fur is present, provided the tongue be closely scraped. The schizomycetes are found in every case in which there is fur upon the surface of the tongue, and I have even found a little of the glœa where no fur was perceptible to the naked eye. The quantity of glœa depends roughly upon the quantity of fur. The position of the glœa corresponds with the position of the fur. The fur dots the tops of the filiform papillæ, and the glœa is attached to the processes of these papillæ. Fresh scrapings of fur show this relation of the glœa to the filiform papillæ, but vertical sections of hardened tongues show more than this. They show that the filiform papillæ are the sole seat of the glœa, which does not exist between the papillæ, and seldom upon the fungiform papillæ. Again, the colour and appearance of the thin grey fur corresponds with the colour and appearance of the thin grey pellicle which forms upon the surface of Bacterium-producing fluids, and as the latter becomes whiter and more opaque as it becomes thicker, so does the fur become whiter and more opaque with increased thickness. A modification of colour is, however, frequently produced by the yellow or brownish-yellow tint of the glœa.

In order to ascertain the true nature of the glœa, and to obtain it in a much purer form than that in which it exists naturally upon the

surface of the tongue, I cultivated it upon a warm stage. Minute portions of fur from different tongues were placed in a drop of aqueous humour, and kept at a temperature of 30° to 33° C. Free growth and development took place, but instead of the single fungus I had expected several fungi were found. Only two forms, however, were present in every instance, namely, *Micrococcus* and *Bacillus*, and, from a comparison of the natural fur with results obtained by artificial cultivation, I think there can be little doubt that the fur consists chiefly or essentially of these two fungi.

Micrococcus existed in every case examined, small spherical bodies generally in pairs or groups of four, but often forming chains. Upon the warm stage rapid multiplication took place with the production of pairs, fours, long and short chains often twisted and looped, and small and large colonies. When these colonies reached a large size (which happened in the course of a few hours) they presented a granular appearance and assumed a yellow or brownish-yellow colour, and all movement ceased in them.* The development of *Micrococcus* occurred abundantly and rapidly in all the experiments made with the exception of one, in which so rapid a formation of *Bacterium termo* took place, that in the course of a few hours the whole of the fluid was clouded and obscured by its presence. Usually the development of other fungi did not interfere with that of *Micrococcus*. Comparing the masses or colonies produced by cultivation with the granular masses of which the fur chiefly consists, the chief constituent of each appears to be the *Micrococcus* sphere. The natural colonies are, of course, not often so pure as those produced artificially, but still not uncommonly these natural colonies present the same regularity of structure as the colonies figured in sketch 5.

The other form, *Bacillus*, was also present in every case examined, but unfortunately development seldom or never occurred, being apparently prevented by the presence of other fungi. It consisted of slender rods, having a well-marked double contour and a light interior. Their length varied much, but was always many times their breadth. There were no defined contents within the rods, except in some of the longer and broader of them, which contained highly refractive spherical bodies which appeared to be spores. The shorter rods moved actively about the field of the microscope, and even some of the longer rods (looking when magnified 450 times from $\frac{1}{2}$ inch to 1 inch long) moved slowly from place to place. The rods were generally straight, but some of the longer ones were curved or bent. They often formed short chains or occurred in pairs, but did not form colonies, although they sometimes occurred in great number and of large size in the *Micrococcus* colonies. They showed very little

* I never observed any lengthening into rods, or the development of any other form from these *Micrococci*.

change in appearance for many hours, sometimes for two or more days, after which they usually became granular and degenerated. These bodies are apparently identical with the *Leptothrix buccalis* of Robin. But I think they would be more rightly called *Bacillus subtilis*. Their length, their slender form, the conditions in which they occur, and the fact of their non-development in the presence of other fungi point to this conclusion. I made many attempts to separate them in order to produce the fungus in a purer form by cultivation, but did not succeed in doing so. Although this fungus did not develop under artificial conditions in the presence of *Micrococcus* and other fungi it is highly, probable that its development takes place freely upon the surface of the tongue. Its habitual presence there, generally in tolerable abundance, and the occurrence of spore-bearing filaments may be adduced as evidence in favour of this view.

Besides these fungi *Bacterium termo* existed in some of the furs examined, and twice developed with such rapidity that the whole of the fluid was crowded with these organisms to the exclusion of every other form. Pairs, chains, and colonies were formed.

Sarcina ventriculi was frequently present and generally developed quickly. It usually occurred in pairs or fours, and was easily recognisable by its large size, compared with the other organisms present, by the square or oblong form of its nuclei, by their faint yellow or red tint, and by the area of protoplasm surrounding the nuclei. The groups of two or four moved slowly about the field of the microscope, but the large masses which were formed remained quite motionless. The masses attained so large a size as seriously to interfere with the growth of some of the other organisms, and when large showed a decided yellow, or brownish-yellow colour.

In two or three of the specimens there occurred rapid and very abundant development of a form of *Spirillum*, which appeared from the double twist which it exhibited, and from its extreme tenuity, to be *Spirochæta plicatilis*. Its growth took place from exceedingly small portions of the organism, and continued only at one end, which was in constant motion, whilst the other end remained stationary; and as the growth progressed, large masses were formed which soon became so dense that it was impossible to discern the nature of the organism of which they were composed. This *Spirochæta* did not occur in most of the specimens examined.

A larger form of *Spirillum* was also occasionally present, but was not seen in the act of developing.

Although I believe the fur consists chiefly of *Micrococcus* and *Bacillus subtilis*, I think it is probable from the results obtained in the experiments upon which the foregoing observations are founded, that the development of these other forms (*Bacterium termo*, *Sarcina ventri-*

Fig. 1.

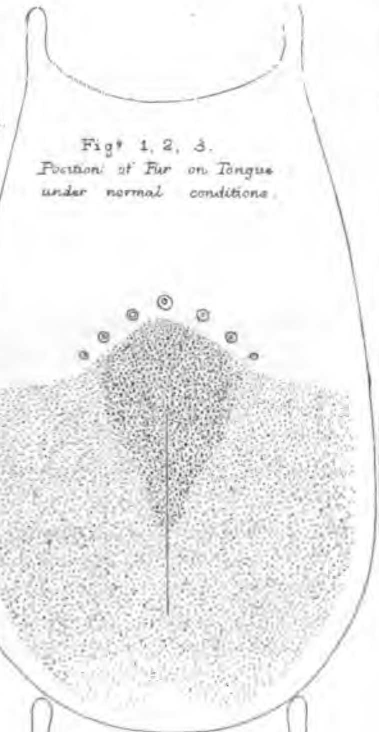


Fig. 2.

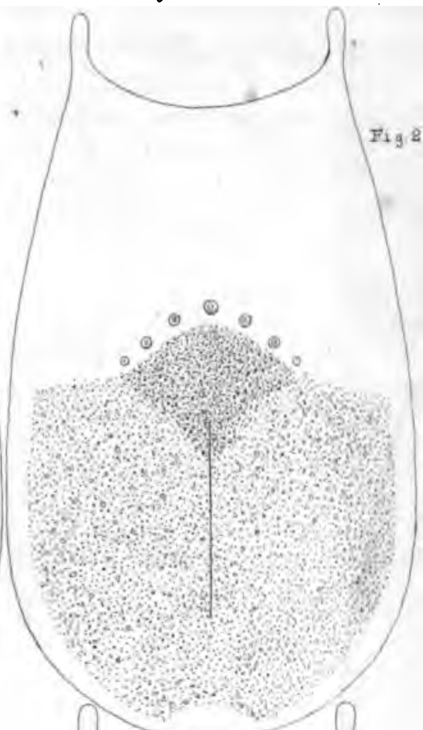


Fig. 3.

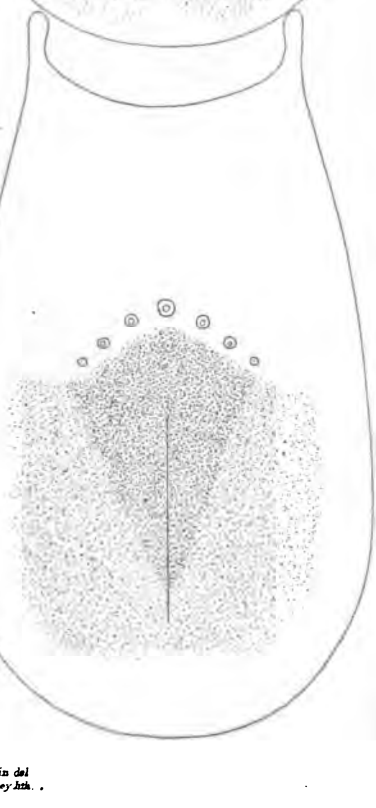
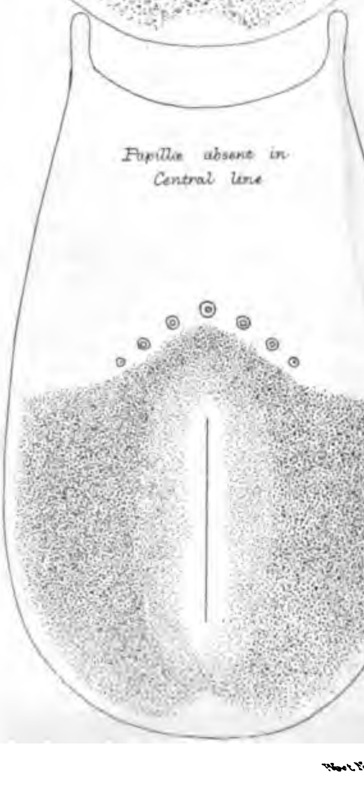


Fig. 4.



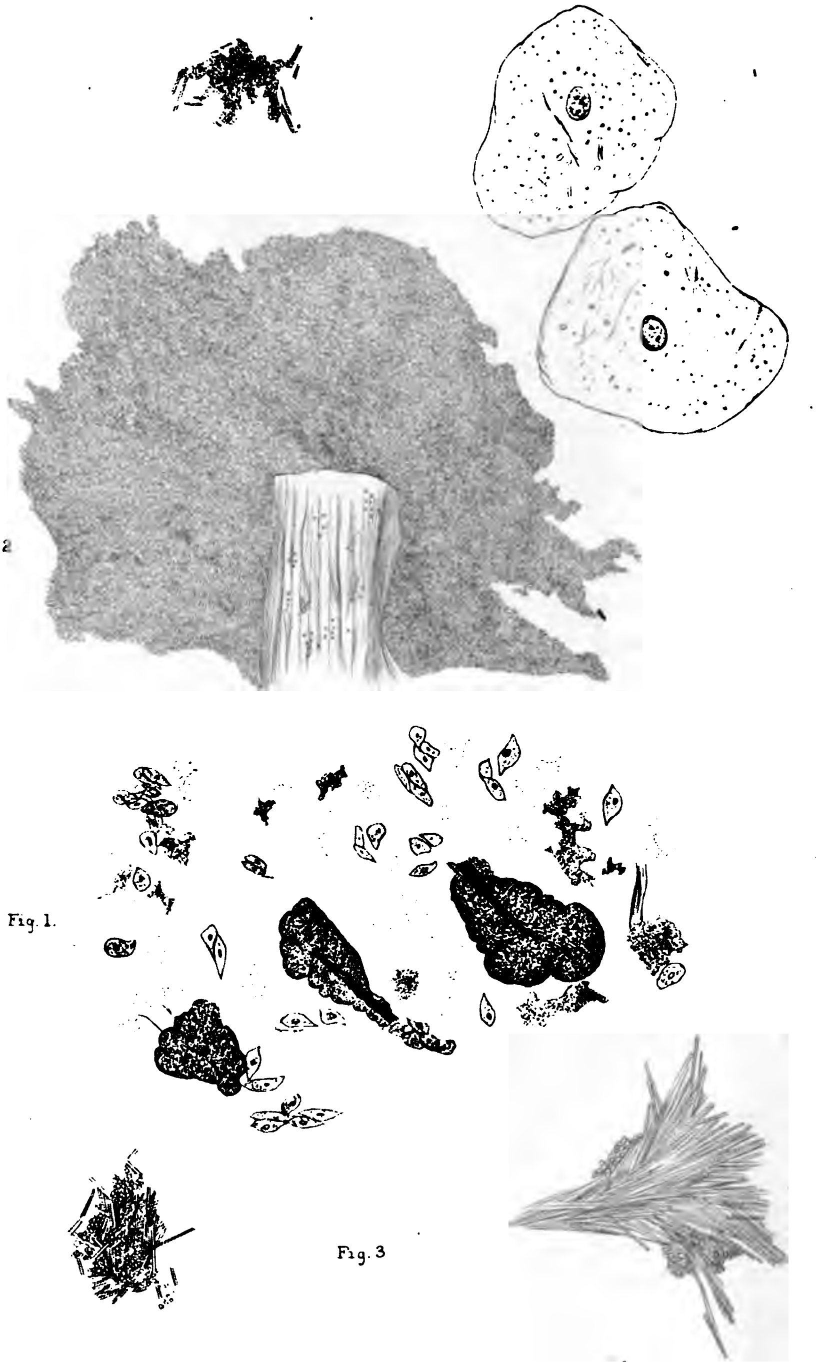


Fig. 2

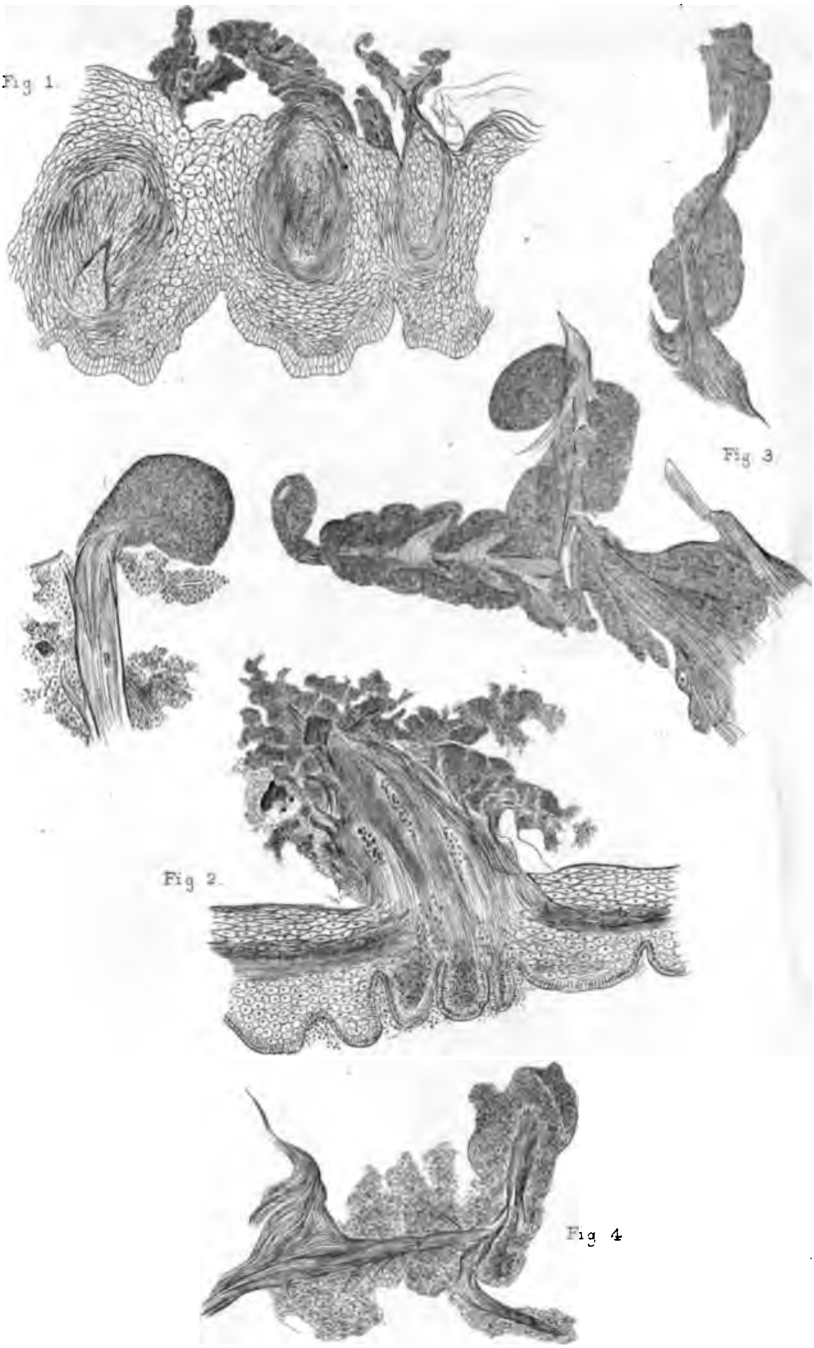
Fig. 1.

Fig. 3

H. T. Butlin del
W.H. Wesley lith.

West Newman & Co imp

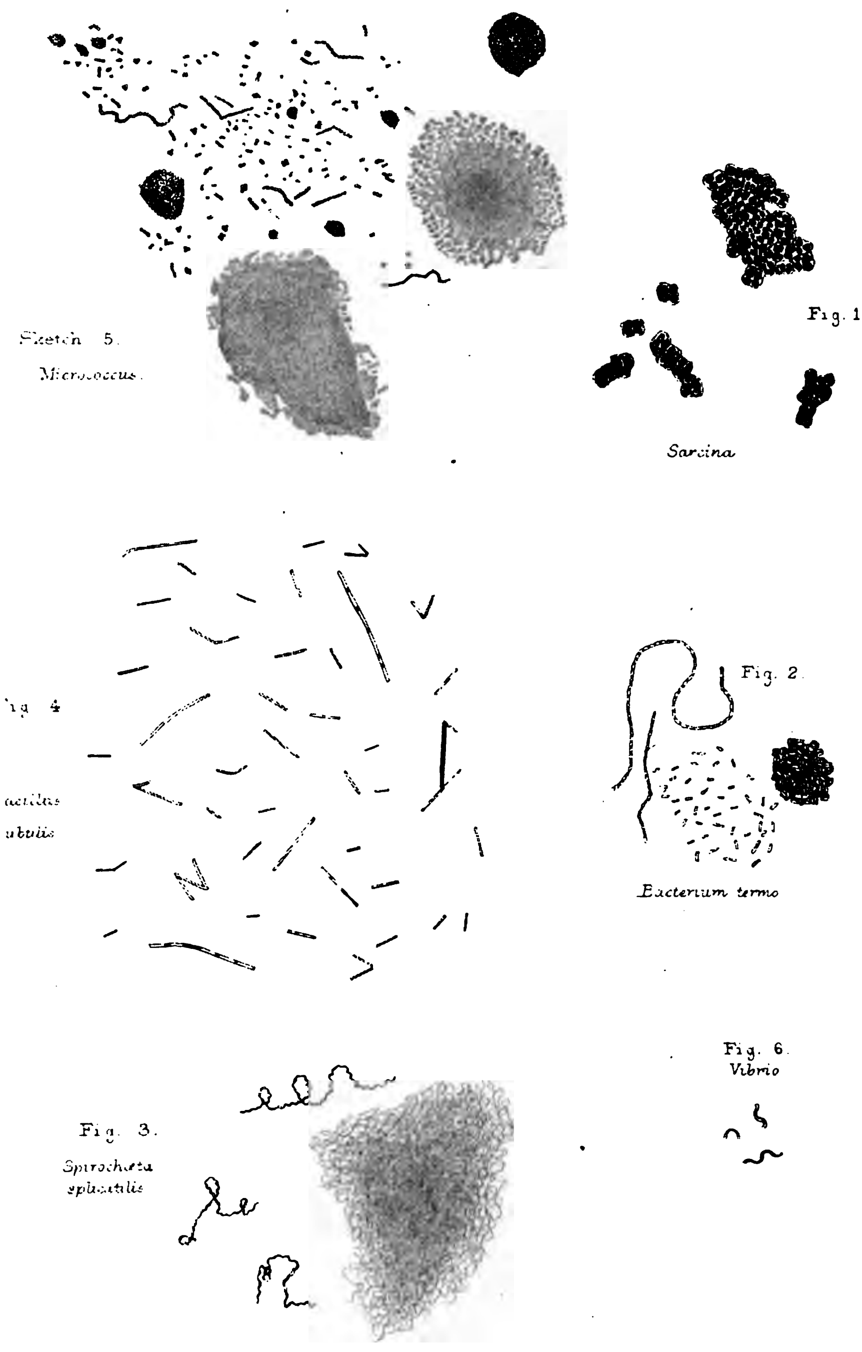
Fig. 1. Constituents of healthy fur (low power)
Fig. 2, 3. d° d° highly magnified. (×450)



H T Butlin del
W H Wesley lith.

West Newman & Co imp.

Figs 1, 2. Sections of Epidermis-fur on filiform papillæ (oc 3, obj 4)
Figs 3, 4. Portions of Papillæ with fur upon them. (× 240.)



Some results of cultivation (×450)

Butler del.
W. ... lith.

West, Newman & Co.

culi, *Spirochœta plicatilis*) may often add considerably to its bulk, and may, perhaps, modify its characters under certain conditions.

The slime which exists around and between the teeth is composed of the same constituents as the fur on the tongue; all the organisms which are found in the one are found also in the other. *Bacillus subtilis* exists, however, in greater quantity in this tooth-slime than in the fur, and the rods and filaments are usually much longer in the tooth-slime, probably because they are not subjected to so much disturbance.

In conclusion I have to thank Dr. Burdon Sanderson and Dr. Lauder Brunton, for valuable suggestions, and for the kindly interest they have shown in this work.

A List of the principal Works relating to the Nature and Character of Tongue Fur.

1831. Piorry. "Du Procédé Opératoire." Paris, 1831.

1845. Remak. "Diagnostische und Pathologische Untersuchungen." Berlin, 1845, s. 221.

1849. Pfeufer. "Der Mundhöhlenkatarrh." Henle u. Pfeufer. Ztchft. f. Rat. Med., Bd. 7, 1849, s. 180.

1850. Miquel. "Untersuchungen über der Zungenbeleg." Prager Viertel Jahrschft., 1850, Bd. 28, s. 44.

1853. Robin. "Végétaux Parasites." Paris, 1853, p. 345.

1861. Neidhardt. "Mittheilungen über die Veränderungen der Zunge in Krankheiten." Arch. d. Wissensch. Heilkunde, Bd. v, 1861, s. 294.

„ Hyde Salter. Todd's "Cyclopædia of Anatomy and Physiology." Art. "Tongue." Vol. iv, pt. 2, p. 1161.

1866. Hallier. "Die Pflanzlichen Parasiten." Leipsig, 1866.

1867. Kölliker. "Handbuch der Gewebelehre." 5th Auflage. 1867. Ss. 348—349.

1873. Fairlie Clarke. "Diseases of the Tongue." London, 1873, p. 93.

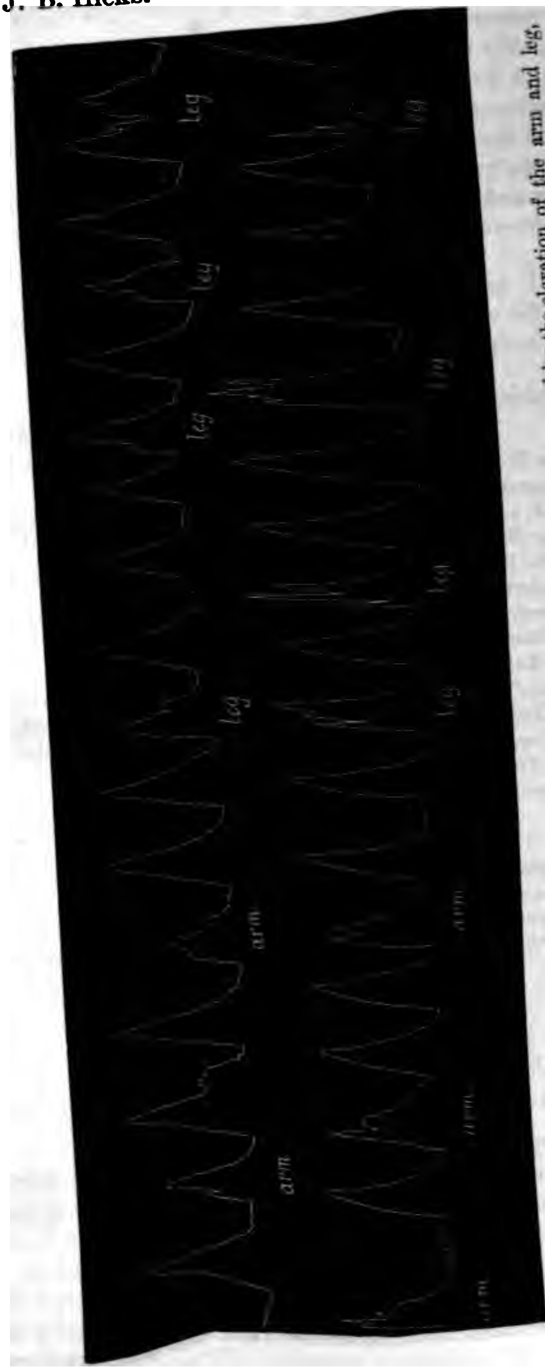
1874. Billroth. "Coccobacteria septica." Berlin, 1874, s. 94.

„ Robin. "Leçons sur les Humeurs." Paris, 1874, p. 550.

1877. Koch. "Untersuchungen über Bacterien." Cohn's Beiträge zur Biologie der Pflanzen, Bd. II, Hft. 3, s. 399.

II. "Note on the Supplementary Forces concerned in the Abdominal Circulation in Man." By J. BRAXTON HICKS, M.D., F.R.S. Received March 26, 1879.

During the ordinary inspiratory effort, the descent of the diaphragm, most noticeable in the male, necessarily produces pressure on the abdominal viscera in contact with its lower surface; these in their turn press down the intestines, which, acting as fluid enclosed in closed elastic sacs, press equally in all directions. Thus during each descent the abdominal walls are projected forwards, as may be readily seen by adapting an instrument similar to a cardiograph resting on three feet,



Tracings of the Abdominal Respiratory Wave in the Male. Showing also the disturbance caused by the elevation of the arm and leg, the person being in the supine position.

the button in the centre attached to the tambour fairly touching the abdominal wall. On revolving the drum attached, a well marked tracing is obtained, showing the respiratory wave; more marked in the male, but almost always well pronounced in the female. The height of these waves, of course, marks the difference of the elevation of the centre of the area and the circle described by the three legs before mentioned, the amount indeed of the bulging of that portion. By this arrangement the effects of the various movements of the body can be also registered with great ease, so far as these movements compress the walls of the abdominal cavity.

But although the abdominal walls in front yield, yet the descent of the diaphragm, which accompanies the inspiratory act, must put pressure on the contents of the abdomen: and thus tension is created, which is in a certain degree lessened—1st, by the yielding of the walls just mentioned, and, 2ndly, by the escape of blood from the vessels within the cavity of the abdomen; and this would be more marked in the case of the venous blood.

In the case of the *arterial blood*, the pressure would tend both upwards and towards the heart, and downwards towards the lower extremities and the abdominal walls. The movement towards the thorax would probably be but slight, yet it would to a certain extent add somewhat to the arterial tension, noticed as commencing at the beginning of inspiration. The other, the downward movement, acting in the direction of the arterial current, would increase also the arterial tension in the lower extremities and abdominal walls. But upon the *venous system* the effect would be greater.

1st. Upon the *systemic* its effects would be cut off in the downward direction by the valves, though this would tend to increase the venous tension in the lower extremities. But this probably would be soon neutralized, or nearly so, by the freedom which the incipient vacuum caused by the expansion of the chest gives the blood to enter the heart. But the pressure caused by the descent of the diaphragm tends to press the blood in the vena cava also upwards, thus facilitating the flow in the natural direction; but any tension to which the vessel is subjected is probably immediately or simultaneously relieved by the suction-action of the chest, which is well known to diminish considerably the blood-pressure in the large veins close to the thorax during the inspiratory movements. That this pressure of the diaphragm on the abdominal contents nearly if not quite balances the suction-action, is shown by the fact that in the sciatic vein the diminution of the blood-pressure during inspiration is not observed.

2nd. The *portal system* is subjected likewise to pressure, and its contained blood would tend to both its incipient and terminal capillaries; and the resultant would be to facilitate its movement towards the area of least resistance, namely, towards the hæpatic veins.

In computing the effect of the descent of the diaphragm we must always bear in mind the effect of the expansion of the lower part or base of the thorax; for this by lifting off as it were the pressure of the abdominal muscles attached to it from the viscera beneath, lessens the effect of the descent of the diaphragm. Notwithstanding this there is a notable residuum of force.

The effect of expiration on the abdominal circulation would be probably to gradually permit a restitution of the balance interfered with. The elasticity of the walls would sustain, to a considerable degree, the pressure; the portal vein and vena cava would gradually accumulate blood, and this in coincidence with an increment in that of the superior cava and right cavities of the heart till the irritation of its presence causes another inspiratory act.

It may be noticed that the tension of the arterial pulse would be naturally increased during the expansion of the lungs, because of the greater supply of blood to the left half of the heart shortly after the commencement of the inspiration, and thus the resistance to the flow of venous blood through the lung capillaries is lessened; and this action it is impossible to ignore when we are discussing the effect of the incipient vacuum on the venous blood-pressure during inspiration.

The same method of registering the effect of the respiratory movements on the abdomen also is applicable to marking the effects of the general movements of the body. The elevation of the arm or the leg, coughing and laughing, &c., are easily seen to compress the abdomen.

It would be beside the intention of this note to discuss the manner in which this effect of movements of the body is produced; but I may point out that in the act of coughing and laughing we have, as indeed might be expected, evidence not only of high pressure (shown by the sudden elevation of the wave), but also a tendency to vacuum, as illustrated by the sudden descent of the wave below the line.

These actions must tell violently on the blood-current of the abdomen, and tend to force it out of this cavity; and, as before remarked, the resultant of this must be to facilitate the current in its normal direction. The same effect must be produced on the other fluids in the abdomen, and must assist the movement of the secretions contained in the ducts of the various organs, notably that of the liver.

(Received April 16.)

In the foregoing note no calculation has been made as to the amount of the forces produced by the descent of the diaphragm in ordinary respiration. Its extreme violent action has been calculated by Professor Haughton at 20 lbs. on the square inch; but the amount of pressure on the contents of the abdomen must vary much, according to the resistance exerted by the parietes. When the intestines are empty of gaseous contents, and the previously over-distended abdomen

is suddenly emptied, as immediately after delivery in woman, this resistance is at the lowest, consequently the effect of descent of the diaphragm on the circulation is but slight, compared with that state which obtains when the parietes are in a high state of health, and the intestines are fully distended with gas, &c.

It must be evident that the amount of blood contained in the vessels within the abdomen must vary much, according to the tension of the parietes; but this matter does not belong to the subject of this note.

III. "Note on the Auxiliary Forces concerned in the Circulation of the Pregnant Uterus and its Contents in Woman." By J. Braxton Hicks, M.D., F.R.S., F.L.S., &c. Received March 26, 1879.

Whatever view we may take of the structure of the placenta, it is generally admitted that both in the large sinuses in the walls of the pregnant uterus, and also in the decidual processes in the placenta as well as in the intervillal spaces the motion of the fluids can be but very slow, that is, if the circulation wholly depended upon the maternal cardiac impulse.

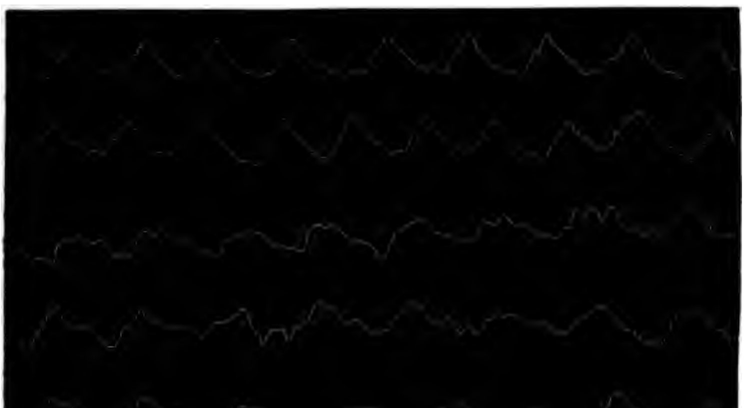
However, in 1871,* I pointed out a fact which had not been before observed, that the uterus was in the habit normally of alternately relaxing and contracting every five, ten, or twenty minutes during the whole of the pregnancy from the earliest period, at least from the second month, and not as had before been believed only under irritation, and towards the end of gestation. This movement is doubtless homologous with the peristaltic movements in the uteri of the lower animals.

In that paper I pointed out—1st, that these movements of the uterus provide for the frequent movement of the blood in the uterine sinuses and the decidual processes; and, 2ndly, that they facilitate the movement of the fluid in the intervillal space of the placenta, or in that which has been called the placenta sinuses, and I remarked, "Whatever view we may hold of the structure of the placenta, whether on the one hand there be blood amongst the villi in maternal sinuses, or on the other merely a serous fluid, in any case it is through one or the other medium the villi absorb the material for the aëration of the fœtal blood; and there can be no doubt that from its position it must be in a more or less stagnant state. It is not difficult, therefore, to recognise the effect which the change in the solidity and shape must produce on the fluids in the placenta, as well as in the uterine walls.

* "Obst. Trans. Lond.," vol. xii. "On the Contractions of the Uterus during Pregnancy: their Physiological Effects and Value in the Diagnosis of Pregnancy."

In other words, these contractions of the uterus act as a kind of supplementary heart to these fluids."

To this force I have now to add the effect of the respiratory movements on the gravid uterus and its contents. Any one who places his hand on the abdomen of a pregnant woman over the centre of the uterus will be conscious of the projecting forward of the uterine wall. But I presume this has been supposed hitherto to be merely the projection of the uterus *en masse*. Admitting that a slight portion of the movement is owing to that, I shall endeavour to show that the much greater portion of the movement is due to the bulging out of the walls by the downward pressure on the fundus during inspiration. This is best demonstrated by a cardiograph constructed with a button tambour, supported by three legs, capable of being adapted by screws to the proper length; these should be as far apart as possible, four inches or so. The patient should be placed on her back, and the tambour tied gently on to the abdomen by a tape passed round the back. The drum being set revolving, the respiratory movement is traced. The respiratory markings are very regular considering the circumstances, interrupted at irregular intervals by the fœtal movements, coughing, and other movements of the body.



Normal Respiratory Wave over Pregnant Uterus. The *sub*-readings depending probably on arterial impulse of mother; and of the fœtus.

Now, it is clear that the readings express the difference of elevation of the uterine wall between the tambour button and the circle enclosed by the legs; in fact, the amount of the bulging of the wall within

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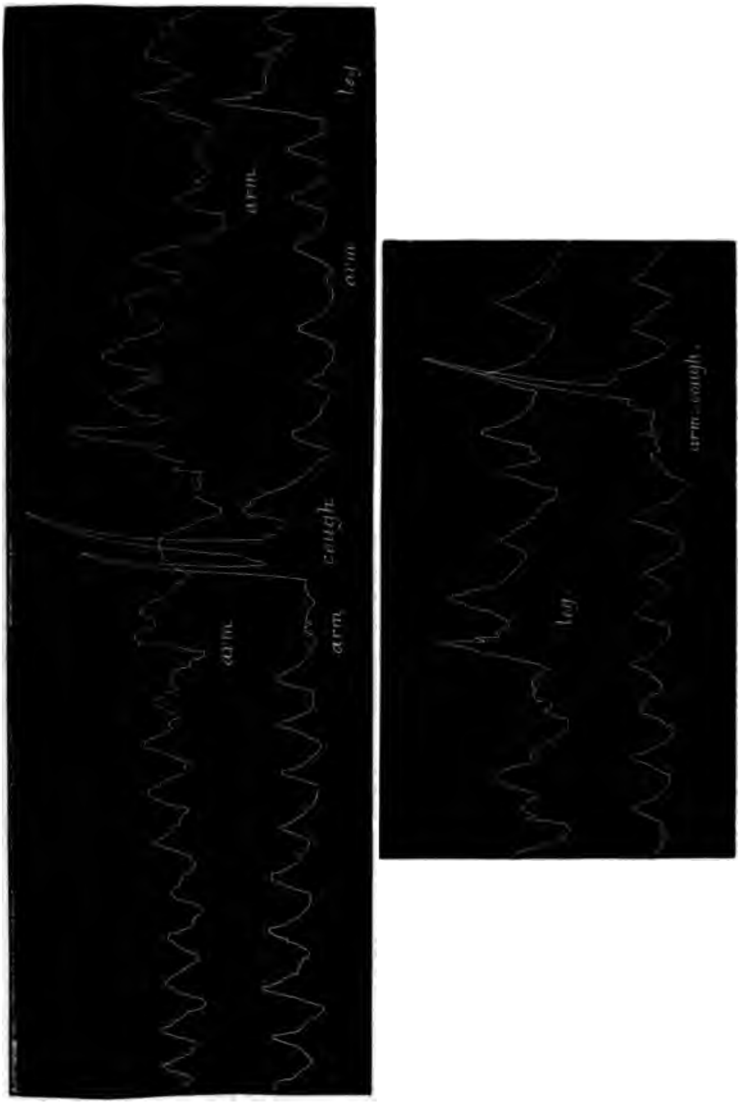
that area. If it were not so, and but the pushing forward of the mass, no difference would exist, and, consequently, no reading obtained. And this is proved by observing the effect on the readings when the uterine contractions occur, to which I alluded at the commencement of this note; for, when these supervene, we find the respiratory readings reduced almost to nothing, and, instead of the high elevation waves of the tracing, well shown before, the line is nearly level. Thus, when the uterus, in consequence of the increased firmness of its walls, cannot be impressed nor can bulge, we have the effect of the descent of the diaphragm to a similar extent reduced. This being admitted, it is clear that every respiratory action causes a movement of the fluids contained within the uterus, thus assisting the circulation in a part apparently removed from the maternal cardiac impetus. It may be worthy of notice, that at the earlier period of pregnancy, the uterine walls are less yielding, and, therefore, less influenced by the respiratory act, but then the assistance this renders at a later period is not so much required, because neither are the sinuses so large nor the decidual processes with their sinuses so deep, nor the thickness of the placenta so great. Gradually as the uterus increases, its walls are more yielding and the force of the respiratory movement more felt within.

The effects of Uterine Contractions during Pregnancy in reducing the height of the Respiratory Wave is seen by comparing the first line with the last. This tracing was taken with a smaller instrument.

There are other points of interest in this registration of the respiratory movements of the abdomen, which do not belong to the subject under consideration, and are, therefore, omitted here.

But there are other accessory forces to be noticed which act on the surface of the pregnant uterus, tending to the movement of the fluids within; namely, the muscular movements of the body, tending to cause a change of shape of the thorax or abdomen. These are quickly shown by the same arrangements as that by which the ordinary respiration is shown. The elevation of the arm, a hoist of the body, and, in particular, coughing, show a much greater force than is exerted by inspiration. Hence, one might fairly infer that exercise

in general in moderation will expedite the flow of the fluids in the uterine vessels, &c., and, also, that a sudden severe action will tend to urge *it* forward so quickly, before the vessels can convey it onward,



Tracings from Abdomen over the Uterus Pregnant at 9th month, showing the ordinary Respiratory Wave, interrupted by movements of arm, leg, and of coughing.

that their rupture would result and effusion of blood be a natural consequence:—a result which experience shows actually occurs under such circumstances.

IV. "A Summary of an Inquiry into the Function of Respiration at Various Altitudes on the Island and Peak of Teneriffe." By William Marcet, M.D., F.R.S. Received March 31, 1879.

On the 19th March of last year, I presented to the Royal Society a short summary of an inquiry on the function of respiration at various altitudes in the Alps. The principal result obtained was that a greater quantity of carbonic acid was formed in the body and exhaled at the higher than at the lower stations. Thus, after experimenting on a spot near the Lake of Geneva, at an altitude of 1,230 feet, and at the summit of the Breithorn, at an altitude of 13,688 feet, there was found to be an excess of 15 per cent. for the carbonic acid expired at the highest station. I had come to the conclusion that the increased formation of carbonic acid in the body at certain altitudes in the Alps appeared necessary, as a means of resisting the influence of cold which is occasionally very great in high Alpine regions.

The question which now offered itself for inquiry was whether, on rising to a considerable altitude above the sea in a warm climate, there would be, as I had found in the Alps, an increase of the carbonic acid expired. After some consideration, the Peak of Teneriffe, in north latitude 28°, was selected as the place best calculated for investigating the subject. The advantages of this site were manifold. First, a mean temperature in the day time, which proved to be not lower than 64° in the shade, could be secured at an altitude above 10,000 feet; next as the mountain rose from the sea, various stations, beginning at the seaside, might be selected; then fine weather could be relied upon in June and July, on the Island of Teneriffe; finally, the spot was situated at an accessible distance from England.

It took me three weeks to collect the necessary instruments, among which was a wooden shed, taking to pieces and made to pack in a comparatively small space. It consisted of six deal boards constructed so as to fit side by side with overlaping edges; when mounted, they formed a flat square roof. The four corners of this roof were supported by four poles held upright by tent ropes and pegs; broad strips of canvas were nailed to two opposite sides of the roof and spread out, being held in position by strings and pegs. The boards covered a square of 6 feet on each side and the sheltered area was much increased by the canvas. The shed was placed lengthways as nearly as possible in the direction of the course of the sun, and by this means we could work all day long in the shade, a necessary condition for the success of the inquiry.

My experimental baggage included two large baskets holding about 150 bottles of a capacity of rather more than 100 cub. centims. each,

and full of a titered solution of barium hydrate, in addition to which there were a number of empty bottles of the same size. The bottles holding the alkaline solution were carefully corked and the corks sealed with paraffin. I must also allude to two strong deal boards or rocking-boards, 6 feet in length and supplied with two iron sockets midway between the two ends; the sockets fitted upon an iron bar raised a few inches high on a firm wooden stand. Two square open wooden boxes were made to fasten at one end of each board respectively, and could be filled with stones or sand up to a given weight. The use of these boards will be explained in the course of the present communication.

In addition to the above apparatus I carried with me a balance and everything required for determining the moisture expired from the lungs. My experimental baggage used in the Alps was also included, together with every requisite for camping out on the Peak for about three weeks.

My Chamounix guide, Edouard Cupelin, who has accompanied me for the last ten years in the Alps, and is thoroughly used to the manipulations connected with my experiments, came out with me to Teneriffe. He not only assisted me most effectually, but also submitted himself to experiment.

We arrived at the Island of Teneriffe on the 25th of June last, and after landing at Santa Cruz, proceeded at once to Puerto de Orotava, at the foot of the Peak. Three principal stations were selected, two at different altitudes on the Peak, and one at the seaside; while from the highest station instruments could be carried to the foot of the terminal cone, and also to the summit of the Peak 12,200 feet above the sea, where I proposed making a few experiments.

We remained eleven days at the lowest station on the Peak, at an altitude of 7,090 feet, and ten days at the higher station 10,700 feet above the sea.

The characters of the stations bearing on my experiments were:—

1. The topographical position and atmospheric pressure.
2. The temperature of the air.
3. The hygrometric state of the atmosphere.

1*st. The position and atmospheric pressure.* My lowest station on the Peak, that of Guajara, was situated on a sandy plateau at the foot of Mount Guajara, known from Professor Piazzi Smyth having established an astronomical station at the summit in 1856. The mountain rose 1,800 feet above my station in the S.W., while in the opposite direction for 200 or 300 yards, there spread a patch of white sand mixed with clay, and baked by the sun. Beyond that could be seen a bank of blocks of lava tumbled over each other, which formed the edge of an upper undulating level reaching the foot of the actual Peak at a distance of two or three miles. The heat of the sun at

that station was intense, as my tent was erected in a hollow, and the sand became so hot in the afternoon that the hand could not bear being kept in contact with it.

The mean of twenty-two readings of a Fortin barometer, by Casella, compared with the observations of Professor Smyth, taken at sea near the coast of Teneriffe, in 1856, at a similar time of the year, or nearly so, gave an altitude of 7,090 feet above the sea for that station. The stations in the Alps where my former experiments had been carried out, and corresponding in altitude with my Guajara station on Teneriffe, were the Riffel (8,425 feet) and St. Bernard (8,115 feet), these however being rather over 1,000 feet higher.

The highest of my principal stations on Teneriffe was that of Alta Vista, where Mr. Piazzi Smyth also resided in 1856. This was near the summit of the Peak on a small "plateau," occurring in a break between lava streams. This station faced an easterly aspect; in the evening a cold westerly wind often blew, sweeping down from the summit and feeling exceedingly chilly. The altitude of this station according to Piazzi Smyth, is 10,702 feet. An accident to my barometer just before leaving Guajara put an end to barometrical readings, but an observation as to the temperature of boiling water at Alta Vista gave me exactly the height as determined by Professor Smyth. This altitude compares well with that of St. Theodule, 10,899 feet, one of my stations in the Alps.

The N.E. trade winds cause a belt of clouds to hover over the island; I entered this layer of fog at an altitude of 3,200 feet, and left it at 5,500 feet, its thickness amounting therefore to 2,300 feet. My stations on the Peak were of course above the clouds; on one occasion only did I see them from Alta Vista make an irruption into the wide plateau at the foot of the peak between 6,000 and 7,000 feet high, but they soon withdrew.

2*nd*, *Temperature.*—The sky was cloudless till the last day, when a few light clouds appeared overhead, and the sun being nearly vertical at noon, in July, its direct heat was very great, although the air was much less warm in the shade; on the other hand the cold was very sharp at night. While the sandy surface of the soil was so hot at two or three o'clock in the afternoon, that the hand could not bear to be pressed against it, water left outside the tent in a bucket or in plates was on several occasions found frozen next morning just before sunrise. I had no black bulb thermometer in vacuo for observing the solar radiation, but Professor Smyth found on the summit of Mount Guajara over 180° F., with such an instrument by half-past nine o'clock in the morning, and he concludes that on August 4th, the black bulb temperature in the sun must have been 212°·4, the thermometer reading in the shade being only 60°, thus leaving the enormous quantity of 152° for the effect of sunshine at

a height of 8,900 feet. (P. Smyth—"Teneriffe—an Astronomer's Experiments.")

Although my first station was 1,810 feet below that at which Piazzi Smyth's observations were made, I cannot think the direct solar heat was notably less.

I procured at Puerto a large box, and had it perforated with many holes on every side to allow of free access of air into it. This box was used as a screen for my thermometers; if I mistake not, a similar plan had been adopted by Professor Smyth. The screen was placed under my wooden shed, and thereby sheltered from the sun. While on my Alpine stations, I was working under a mean temperature of 39° at St. Theodule, and 52° and 43° at the Riffel and St. Bernard respectively, my atmospheric temperature on the Peak of Teneriffe was from 65° to 69° in the shade, and rose in the sun much higher than on the Alps; in fact I was throughout the day time exposed to a climate much warmer than at my Alpine stations; so far, therefore, my object, in going to Teneriffe, of avoiding cold at comparatively great altitudes above the sea was attained.

3rd, Moisture.—The great dryness of the air in the daytime was very remarkable, the total mean difference between the dry and wet bulb readings at Guajara (7,090 feet) being 25°·6, and at Alta Vista (10,700 feet) 19°·7; while at Puerto de Orotava, at the seaside, the difference fell to 8°·7. I was never conscious of perspiring, and my skin was always very dry, with the throat parched at times. The evaporation from the skin must have been very great so high above the sea, in such dry air and under so powerful a sun.

The inquiry may be divided into three parts: The first refers to the respiratory phenomena at the various stations while in the sitting posture. The second, to the respiratory phenomena observed while engaged upon a definite amount of muscular work. The third, to the amount of watery vapour expired sitting at my different stations. I shall beg to commence with the experiments relating to the breathing while in the *sitting posture.*

The method adopted in these experiments was precisely the same as that I had made use of in the Alps, with this very slight difference, that instead of cooling the air expired into the bag, to the temperature of the water in the aspirator, where it was treated with the solution of barium, I noted the temperature of the air in the bag immediately after filling it, and drew the air at once from the bag into the aspirator or tube, recording its temperature in the tube. In nearly every case the temperature in the tube was rather lower than in the bag, so that a contraction took place; the degree of contraction was duly taken into account in the calculations of the analysis. I also used common water instead of a solution of salt for aspiring the air for analysis into the tube.

The air from the lungs was expired into a strong india-rubber bag of a known capacity under a pressure of one inch of water. The bag used in nearly every experiment *sitting* held 39·3 litres of air under that pressure; and in the experiments made while engaged with a measured amount of muscular work, a bag holding 68·4 litres of air under the same pressure was employed.

The tube into which the expired air was drawn for analysis was supplied with the india-rubber diverticulum described in my former communication, and I made occasional use of it to take out small quantities of air and test them with a solution of barium hydrate. I thus observed that a continued agitation of five minutes sufficed for the entire combination of the carbonic acid. In every experiment the agitation was continued for six or seven minutes or longer, by the watch. The bottles, into which the fluid was drawn after agitation, were well corked, and their necks dipped into melted paraffin. Although large enough for somewhat more than the bulk of the fluid they contained, the empty space was too small for the air it held to affect the alkaline solution.

My Chamounix guide was practised in the mode of breathing into the bag, so that I could rely upon his doing this in a perfectly natural way, and without the loss of any of the air expired; he was also in the habit of counting his expirations while so engaged.

We assisted each other mutually; one of us keeping an eye on the stop-watch and the bag, while the other was breathing into it. After sitting quiet for a few minutes, the mouth was applied to the mouthpiece, and at the very beginning of the first expiration, a sign was made and the stop-watch started. When the bag was nearly full, the water in the gauge began to rise, and the instant it attained the height of one inch, the watch was stopped. The time to fill the bag was then read off, and the temperature of the air in the bag ascertained, both observations being immediately noted. Without any loss of time the air was at once aspired into the cylinder, and its temperature within the cylinder again read off by means of a thermometer run through the india-rubber stopper.

Then followed the introduction of the normal alkaline solution, the agitation and the bottling; a whole experiment took from thirty minutes to forty-five or fifty minutes. The total number of my Teneriffe experiments on respiration, including the determination of the carbonic acid expired, amounted to 157.

The Chamounix guide is a tall and very powerful man of 38 years of age; I found him to measure round the bare chest at the nipples, 3 feet 5 inches. His height, in boots with moderately thick soles, is 6 feet 0¼ inch, and he subsequently found his weight to be 89 kilog., —exactly 14 stone.

I am 50 years of age, measure 2 feet 10½ inches round the bare

chest, have a height in boots with moderately thick soles of 5 feet 7$\frac{3}{4}$ inches, and weigh 70 kilog., say 11 stone. We are both in the enjoyment of very good health.

It will be observed that we lived precisely in the same way, were exposed to the same kind of atmospheric influence, and ate the same kind of food, although from the weight of his body, the guide consumed more than I did. The amounts of carbonic acid we expired could therefore be fairly compared with one another.

The mean weight of carbonic acid expired from sixty experiments for myself, and fifty-five for the guide, both sitting, and at the same stations respectively, was in my case, 472 mgms. per minute, and in that of the guide 604 mgms., or on 100 kilos. weight of my body, I expired 674 mgms. of carbonic acid per minute, and the guide also on 100 kilos. weight, 679 mgms. Thus it was found that we both gave out at the lungs an amount of carbonic acid proportional to the weight of our body. This is an interesting, though not unexpected result, which appears to me to give much weight to the correctness of the investigation, and consequently to the reliability of the conclusions.

Another circumstance in connexion with the present work still more deserving of notice than the former, was the fact that while we were engaged raising at each step a weight of 39·5 lbs. with the feet, on rocking boards, at the rate of 45 steps per minute, as will be subsequently described, a mean amount of carbonic acid was expired by each of us respectively, again proportional to the weight of our body. In these experiments, the mean weight of carbonic acid obtained for myself from eighteen experiments, six at three different stations, was 1·011 grms. per minute, and for the guide from the same number of experiments at the same stations 1·269 grms., giving for myself for 100 kilos. of body, 1·444 grms., and for the guide for 100 kilos. of body 1·426. Nothing can be more conclusive; we again produced within our bodies as nearly as possible the same amount of carbonic acid proportionally with our weight. These figures also show that the method adopted was well calculated to give reliable results, while engaged in a definite amount of muscular exercise.

Amount of Carbonic Acid expired at the different Stations.

The mean amount of carbonic acid expired at the several stations by both of us in the sitting posture, was found, to a great extent, to be influenced in a similar way by the food taken. In both cases, with but one exception, the greatest amount of carbonic acid expired was during the first or second hour after eating, and the quantity diminished as time elapsed from the last meal taken.* The exception

* Dr. Edward Smith's ("Phil. Trans.," 1859) experiments show that a minimum amount of carbonic acid expired is obtained while fasting, beyond which continued fasting, within certain limits, produces no further reduction.

[redacted]

refers to the guide at Puerto, where his maximum is found to be during the third hour after a meal. The fluctuations in my case may be said to follow closely those formerly reported from my experiments in the Alps.

The subjoined table shows at a glance the variation of the mean amount of carbonic acid expired during each successive hour after food, the fifth or sixth hour being grouped together for want of a sufficient number of experiments.

Table showing the Influence of Food on the Expiration of Carbonic Acid at the various Stations (in the sitting posture).

Self sitting.

Hours after Food.	Alta Vista. CO_2 expired per minute.	Guajara. CO_2 expired per minute.	Puerto. CO_2 expired per minute.
0 to 1 hour	0·534 (3)	0·374 (1)	0·467 (3)
1 „ 2 hours..........	0·502 (8)	0·497 (6)	0·496 (5)
2 „ 3 „	0·472 (5)	0·486 (4)	0·498 (6)
3 „ 4 „	0·435 (4) (3 to 6 hours)	0·424 (6)	0·448 (4)
4 „ 6 „		0·398 (2)	0·384 (2)
Cupelin sitting.			
0 to 1 hour	0·604 (5) (0 to 2 hours)	0·560 (2)	No experiments.
1 „ 2 hours..........		0·609 (5)	0·684 (5)
2 „ 3 „	0·570 (7)	0·560 (6)	0·711 (6)
3 „ 4 „	0·525 (4)	0·565 (4)	0·684 (5)
4 „ 5 „	No experiments	0·489 (4)	0·609 (2)

The figures between brackets refer to the number of experiments. One experiment, at 5.48 A.M. at Guajara, not included.

If the figures reported in this table be taken into consideration together with the corresponding results obtained in the Alps, it will appear that the maximum amount of carbonic acid is expired rather earlier after a meal on the mountains than in the plains, which would show that there is apparently a tendency to a more rapid digestion and assimilation of food in the mountains than near the sea level.

As in the case of my former investigation, I have neutralised as much as possible the influence of food on the results of the experiments, by conducting the inquiry at all times of the day between breakfast and bedtime.

Influence of Temperature on the Carbonic Acid expired.—So far, to my knowledge, the only series of observations we possess on the influence of tropical climates on the functions of the human body, are those of Dr. Rattray, Surgeon R.N., who has clearly taken great pains to investigate the subject; he concludes that:—

"The three marked tropical phenomena, viz., diminished lung-

vascularity, slower respiration, and gentler breathing are closely related, and together indicate reduced lung work, the reverse for the temperate zone marking an increased function,* &c."

Dr. Rattray infers, without apparently making any actual determination of carbonic acid in the air expired, that there is a larger amount of carbon thrown out by the lungs in temperate than in tropical climates. The consideration of the mechanical action of heat, with reference to the functions of the body, had led me long ago to adopt the same views; and previous to my Teneriffe experiment, I had believed that where the heat of the sun was in excess, less heat was required to be manufactured by the body for the due performance of its functions, and, consequently, less carbonic acid was formed and given out. I am now compelled, however, to alter this view, and to conclude that more carbonic acid is formed in the body under a tropical or nearly tropical sun than under temperate latitudes.

In order to make the subject perfectly clear, I have placed, in a tabular form, the figures showing the amount of carbonic acid expired, as found by direct experiments both in my Alpine and southern stations.

Table showing comparatively the Weights of Carbonic Acid and the Volumes of Air (reduced) expired per minute by myself and guide in the Alps and at Teneriffe.

Self sitting.

Stations.	CO_2 expired.		Volume air expired per min. (reduced to 32° and seaside pressure).		Number of experiments.
	Grm.	Increase for Teneriffe. Per ct.	Litres.	Increase for Teneriffe. Per ct.	
Alta Vista........	0·486	13·8	5·14	7·8	20
Breithorn and St. Theodule.......	0·419		4·74		23
Guajara..........	0·458	9·6	5·47	16·1	20
Riffel and St. Bernard...........	0·414		4·63		29
Seaside, Puerto ...	0·471	18·7	5·84	12·0	20
Lake of Geneva ...	0·383		5·14		37
		Cupelin sitting.			
Alta Vista...... Guajara........	0·564	Nil	6·24	5·8	37
St. Bernard	0·565		5·88		4
Puerto.	0·685	17·5	7·71	23·7	18
St. Bernard.......	0·565		5·88		4

The figures for the weights of CO_2 expired in the Alps have undergone a correction. See foot-note, page 507.

* "Proc. Roy. Soc.," vol. xxi, 1872.

It will be observed in this table, that, in my case, when approximately equal altitudes in the Alps and on the Peak of Teneriffe are compared as to their influence on respiration, at the highest stations there is an increase of carbonic acid expired by 13·8 per cent. for Teneriffe; at the stations next in altitude, the increase is by 9·6 per cent. for Teneriffe, and at the seaside, compared with the shores of the Lake of Geneva, the enormous increase for Teneriffe of 18·7 per cent. is noted. As to my guide, I have, unfortunately, but few experiments on the carbonic acid he expires on the Alps, which only amount to four in number. They show for approximately equal altitudes no increase of carbonic acid expired on the Peak of Teneriffe; but at Puerto de Orotava I find him to give out a very large quantity of carbonic acid in excess of that he expired in the Alps, amounting to as much as 17·5 per cent. There are no determinations of the carbonic acid expired by the guide at the altitude of Geneva, to compare with those obtained at the seaside on the Island of Teneriffe, but the increase at Teneriffe is greatly beyond any result that might have been expected at the lowest northern station.

If my excess of carbonic acid expired on the Peak of Teneriffe, over the amount expired in the higher Alps amounts to 13·8 per cent., while there is no increase in the case of the guide, this is probably owing to the guide apparently perspiring much more freely than I do, and to the circumstance that his home is in the mountains, while I am accustomed to a residence at the sea level.

This fact, that an excess of carbonic acid is expired in hot climates over that given out in temperate zones, is to me so unexpected, and, indeed, so different from what might have been anticipated, that I feel bound to give every possible proof of the accuracy of my work.

An objection might be raised to the correctness of the analysis from changes occurring in the normal solution of barium from the action of the carbonic acid of the air. This was carefully guarded against; the whole contents of one small bottle were used for each analysis, thus avoiding the necessary introduction of air in opening the bottle had the stock of the alkaline solution been carried in a single large flask. The normal solution was seen to be perfectly clear when poured into the 100 cub. centim. pipette, although it had travelled all the way from London to Teneriffe, and been carried on mule-back to near the summit of the Peak. But a circumstance still more convincing of the satisfactory state of the solution of barium was derived from the examination of a bottle of this solution, which had accidentally escaped being used at Teneriffe, and was found after my return on unpacking the basket. The solution in this bottle exhibited a small number of white specks at the bottom, there were so few that on shaking the solution looked clear; on standing the specks reappeared. I subjected this fluid to a careful analysis. 25 cub. centims.

mixed with 100 cub. centims. of distilled water gave, in order to neutralize 5 cub. centims. of the oxalic acid solution, 9·00 cub. centims. as the mean of six determinations. My normal solution of barium, analysed in London before leaving for Teneriffe, had yielded 8·92 cub. centims.; the difference was only by 0·08 cub. centim. This result would give a very slight deficiency of carbonic acid, but the error might be expected to correct itself in a number of experiments.

Finally, it might be objected that, in my Alpine experiment, a loss of carbonic acid had been experienced from the india-rubber bag into which the expired air was collected. In these experiments a certain time elapsed after filling the bag previous to the air it contained being introduced into the tube; this lapse of time ranged between a few minutes and thirty-five or forty minutes, and was required to allow the air in the bag to cool down to the temperature of the water in the tube. No doubt, after a certain time, an escape of carbonic acid might be expected to take place through the substance of the india-rubber bag, but no such escape, in any appreciable degree, could have occurred during the above-mentioned period. This I determined experimentally by subjecting a sample of expired air to analysis immediately after filling the bag, and another sample of the air from the same bag some time later. The results from four analyses made at Cannes in February (1879) were as follows:—

Experiment.	CO_2 expired per minute.	Time bag was exposed to the air.	CO_2 found after waiting.	Difference per cent.
1	0·451	25 minutes	0·454	0·66 more.
2	0·411	27 „	0·407	0·97 less.
3	0·382	50 „	0·380	0·52 „
4	0·462	30 „	0·469	1·5 more.

It is, therefore, obvious that, in my experiments on the Alps, no appreciable loss of carbonic acid through the substance of the bag took place previous to the air being subjected to analysis.*

* In the whole of these experiments the air had been aspired into the tube for analysis by means either of a nearly saturated solution of common salt or of water. It had not occurred to me, at first, that the fluid adhering to the inside of the tube would have a material influence on the volumetric analysis which was to follow. I determined the mean volume of fluid thus left in the tube, from 14 experiments, to amount to 3·3 cub. centims.; an error thus crept into the analysis, which, though not interfering with the results as to the carbonic acid expired in the Alps, relatively to each other, had, however, to be corrected when these results were compared with those obtained at Teneriffe. It was calculated for every experiment separately both in the Alps and at Teneriffe, and the correction was made accordingly. This work proved very laborious, and delayed considerably the completion of this paper. There is another probable slight source of error to be noticed in the analysis con-

It is known, from Dr. Rattray's important researches, alluded to above, that the body loses weight by a change from a temperate to a tropical climate, and recovers its weight on returning into a colder latitude, the loss appearing independent of the amount of food taken. This falling off in the substance of the body, attended, as I have shown, by an increased formation and expiration of carbonic acid, must be due to increased combustion or excessive oxidation.

It is difficult to offer a theory to explain this phenomenon in our present knowledge of the action of heat on the living body. Cold we know to increase the amount of carbonic acid formed in the body, the object of which is clearly to keep up animal heat to its normal standard; it is odd indeed that an increase of external heat should exert a similar influence. I do not think it necessary to do more than allude to a tendency to looseness of the bowels I had while on the Island of Teneriffe, which I ascribe to the heat of the climate; the guide informs me there was an opposite disposition with him at Puerto, on the seaside. These minor circumstances interfered in no way with our health, which was quite good, and our work was continued nearly daily, and all day long, during our stay at Teneriffe.

The following table gives the result, in a condensed form, of the whole of my inquiry on respiration at Teneriffe, in the sitting posture. (See p. 509.)

The chronological order of my visits to the several stations was—

1. Guajara.
2. Alta Vista.
3. Foot of Cone.
4. Puerto de Orotava (seaside).

The number of experiments made sitting amount, for myself, to 65, for the guide to 55, making altogether 120, and, in each of them, a sample of air expired during from four to six minutes was analysed. The titrations were subsequently all made by myself near Geneva, in the open air, on a balcony, and in order to guard against any accidental mistake in the calculations of the analysis, they were all done by myself and an assistant conjointly.

There was but a very slight increase in the carbonic acid expired at the two highest stations beyond the amount given out at the seaside, and it bore no comparison with the excess of carbonic acid expired at a similar altitude above the sea in the Alps. The mean excess of the

nected with the Alpine experiments, though not with those of Teneriffe, and owing to the circumstance that the solution of common salt used, probably contained a small quantity of alkaline sulphate, the alkali set free by the action of the barium exerted an influence on the titration, apparently increasing the amount of carbonic acid present. Experiments made with three different samples of common salt showed me that the error may safely be limited to 3 per cent., and is certainly much less in many instances.

Table showing the Mean Results from Experiments at Teneriffe in the Sitting Posture.

Stations.	Atmospheric pressure.	Altitude.	Mean temperature during experiments.	Weight of CO_2 expired per minute.	Volume of CO_2 (reduced) expired per minute.	Volume air expired per minute, *not reduced.*	Volume air expired per minute *reduced.*	Per cent. CO_2 in air expired by volume.	Frequency of expiration per minute.	Vol. air expired per expiration, not reduced.	Number of experiments.
		Feet.	Fahr.	Grms.	Litre.	Litres.	Litres.			Litre.	
					Self.						
Foot of Cone	506 mm., 19·922 ins. (assumed).	11,745	64°	0·471	0·239	8·04	4·99	4·9	10·0	0·81	5
Alta Vista	521 mm., 20·513 ins. (Piazzi Smyth).	10,700	64·2	0·479	0·245	8·07	5·14	4·8	11·4	0·71	20
Guajara	594 mm., 23·397 ins. (self).	7,090	69·6	0·458	0·234	7·62	5·47	4·2	11·4	0·67	21
Puerto, seaside	760 mm., 29·922 ins. (taken at).	..	75·7	0·471	0·239	6·44	5·84	4·1	7·7	0·79	20
					Cupelin.						
Alta Vista	521 mm., 20·513 ins.	10,700	66·5	0·568	0·290	10·19	6·47	4·4	10·6	0·96	16
Guajara	594 mm., 23·397 ins.	7,090	69·0	0·560	0·285	8·45	6·07	4·7	10·6	0·81	21
Puerto, seaside	29·922 inches	..	76·2	0·685	0·359	8·51	7·71	4·6	7·9	1·07	18

two highest stations on Teneriffe, above the amount expired at the seaside, is only 1·2 per cent., which is so small as to be hardly worth recording. In the Alps, at altitudes somewhat corresponding with those of the Teneriffe station, but in a much colder climate, the excess of carbonic acid expired at the highest over the lowest station was 15 per cent., while, if the mean of the four high stations over the fifth or lower station be taken, it will give an excess of 8·1 per cent.

In the case of the guide, there is not only no increase of carbonic acid expired in the high stations, but we find a considerable increase at the lowest station (above the two others), where the heat felt, and consequently absorbed, was the greatest; this increase amounts to 17·8 per cent.

The mean volume of air expired, reduced to 32° and the seaside pressure, was observed in my case to fall by 14·5 per cent. from the lowest to the highest station. With the guide there is also a decrease of air expired under similar circumstances by 16·1 per cent.

I find the percentage of carbonic acid in the air expired to increase in my case from 4·1 per cent. at the lowest station to 4·9 per cent. at the highest, while with the guide the proportion of carbonic acid in the air exhaled is nearly the same at his three stations.

The frequency of my respiration undergoes a marked reduction at the seaside, though nearly the same at my three high stations; the reduction amounts to no less than 31·2 per cent. In the case of the guide, the mean number of respirations per minute is exactly the same at his two high stations, but also falls off at the seaside by 25·5 per cent.

In all these experiments air was breathed through a mouth-piece, and on that account the rate of breathing was a little slower and apparently rather deeper than if no mouth-piece had been used. The same method was pursued in every experiment, so that the results may be compared with each other with all due regard to strict accuracy.

Respiration during a Measured Amount of Muscular Exercise.

In my former communication, I related a certain number of experiments referring to the increased expiration of carbonic acid while in the act of ascending. Since then it occurred to me that an inquiry into the amount of carbonic acid expired during a well-regulated walking exercise would yield interesting results. From the difficulty of regulating exactly the degree of muscular power exerted while walking, it occurred to me that some arrangement, on the principle of a tread-wheel, was more likely to answer my purpose, and I finally adopted the tread-boards or rocking-boards described at the beginning of the present communication. While using these boards we raised a weight of 39·5 lbs. forty-five times per minute, as measured by a metronome, to a height of 5·06 inches for every step.

Before collecting the air expired, the boards were worked at the rate of forty-five steps per minute for a short time, in order to bring the body thoroughly under the conditions of the experiment.

The 68·4 litre bag connected with the water gauge was held by the hand in the proper position, and at the same time as the first expiration into the bag was commenced a preconcerted signal caused the assistant to start the time-piece. A little practice made it quite easy to step in time with the beats of the metronome, counting the number of expirations. As soon as the water gauge showed a pressure of one inch, the watch was stopped and the number of expirations immediately recorded.

The experiment was then completed as usual.

Six experiments were made by each of us at the different stations, and the results are entered in the following table:—(See p. 512.)

On considering in this table the amount of carbonic acid exhaled, it will be observed to vary but little at the different stations for both myself and the guide respectively. In my case the amount expired at 10,700 feet and seaside is nearly the same, while there is a moderate increase at Guajara, the intermediate station. In the case of the guide the amount expired at the two highest stations is much alike, and there is a moderate decrease at the lower station.

The proportion between the mean carbonic acid expired sitting and on the rocking-boards for each of us respectively at the various stations were:—

	For myself.	For the guide.
Alta Vista	1 to 2·05	1 to 2·23
Guajara	1 „ 2·35	1 „ 2·35
Puerto	1 „ 2·06	1 „ 1·78
Mean	1 „ 2·15	1 „ 2·12

Consequently, the proportion of CO_2 is a little higher for both of us at the intermediate station, while the total mean in each case is as near as possible identical, and may be safely considered as the same. These figures show, moreover, that while engaged with the regulated work on the tread-board, we each of us expired nearly twice as much carbonic acid as in the sitting posture.

The mean volume of air expired per minute, reduced, is for myself considerably smaller at the highest station than at the two others; while in the case of the guide we observe a slight falling off in the volume of air expired at the middle station.

If the relation between the volume of air and weight of carbonic acid expired for each of us at all the stations be calculated, it will be found that for myself 1 grm. of carbonic acid (expired on the tread-

Table showing the Mean Results from Experiments made on the Rocking-boards.

Stations.	Atmospheric pressure.	Altitude.	Mean temperature during experiments.	Weight of CO_2 expired per minute.	Volume CO_2 reduced expired per minute.	Volume air expired per minute, not reduced.	Volume air expired per minute reduced.	Percentage CO_2 in air expired (by volume).	Frequency of respiration per minute.	Volume air expired per expiration, not reduced.
	Inches.	Feet.	Fahr.	Grms.	Litre.	Litres.	Litres.			Litre.
					Self.					
Alta Vista	20·513	10,700	61°	0·986	0·502	15·14	9·72	5·1	13·0	1·17
Guajara	23·397	7,090	74·6	1·076	0·547	17·67	12·60	4·4	12·9	1·37
Puerto............	29·922	Seaside	75·2	0·971	0·494	13·60	12·36	4·0	11·3	1·21
					Cupelin.					
Alta Vista	20·513	10,700	61	1·270	0·647	22·06	14·27	4·5	11·5	1·92
Guajara	23·397	7,090	74·4	1·315	0·669	18·43	13·14	5·1	12·6	1·46
Puerto............	29·922	Seaside	75·0	1·221	0·622	15·88	14·44	4·3	9·0	1·76

boards) corresponded to 1·20 litre of air, while with the guide 1 grm. of carbonic acid corresponded to 1·48 litre.

As the mean results obtained for the amount of carbonic acid expired sitting and while on the tread-boards, agrees so well with both of us respectively, I have thought it worth while to calculate the mechanical power developed by the combustion of the amount of carbon burnt while working the tread-boards, in excess of that consumed in the sitting posture. 17·92 kilos. were raised to a height of 128·5 millims., 45 times per minute.

	Per 100 kilos.
Mean carbonic acid per minute on the tread-board	1·435
„ „ sitting	0·676
Excess „ expired on the tread-boards	0·759 grm.

Corresponding to 103·6 kilogrammetres $(0·1285 \times 17·92 \times 45 = 103·6)$ of work done, or 0·00733 $(103·6 : 0·759 = 1 : x)$ CO_2 expired, was equal to an oxidation of 0·002 grm. carbon, capable of raising 1 kilo. to 1 metre.

From Watts' Dictionary of Chemistry (vol. iii, pp. 105 and 129) the mechanical action of one unit of heat=423·5 gramme-metres, and one gramme of carbon yields by its combustion 8080 units of heat. Therefore, 1000 grammes carbon=8080000 units of heat yielding (8080000 × 423·5) 3421880000 gramme-metres or 3421880 kilogrammetres for the mechanical action of 1 kilogramme of carbon.

The relation between the above theoretical mechanical power of burning carbon and the actual mechanical power found to be evolved in my experiments was as follows:—

$$1000 \text{ grms.} : 3421880 = 0·002 : x. \quad x = 6·84.$$

Therefore we only applied $\frac{1}{6·84}$, or 0·147 of the power the carbon we burnt on the tread-boards (in excess of that consumed sitting) was theoretically able to exert.*

As to the percentage of the carbonic acid in the air expired, while on the tread-boards, it increases at the highest station in my case, and this increase is somewhat gradual from the lowest to the highest station. With the guide the maximum percentage is met with at the middle station.

The frequency of the respiration increased in my case from the lowest to the highest station, while with the guide it is slightly in-

* There is so little carbonic acid present in the atmosphere, especially at some altitude above the sea (M. P. Truchot, "Compt. Rend. de l'Académie," vol. lxxvii, 1873), that its presence in the air breathed has not been taken into account in this calculation.

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creased at the middle station, undergoing a marked and sudden reduction at the seaside.

Water Expired from the Lungs at the Various Stations.

The third part of my paper refers to the moisture exhaled.

It was apparent at the outset that a falling off in the atmospheric pressure, from rising above the sea, would be attended with a corresponding increase of evaporation from the lungs, and a proportional cooling effect on the respiratory organs. The apparatus used for the inquiry was disposed as follows:—

A tube drawn out at both ends was loosely filled with fragments of calcic chloride; it was large enough to ensure the absorption of the whole of the vapour expired in three minutes. One end of the tube was connected with one of my large india-rubber bags, while the other end had a ring of vulcanised india-rubber fixed round, to which the mouth was applied. A delicate spring valve (by Coxeter) was fitted into the neck of the tube next the bag, and was weighed with the tube; it effectually prevented any admission of air into the tube except that given out from the lungs. Either the tube or the bag was placed in communication with a water gauge by a neck and india-rubber tubing. Every now and then the calcic chloride was tested as to its power of retaining all the moisture; this was done by connecting another similar tube with it and weighing it after breathing through them both. No mouthpiece was used in these experiments, as moisture was found to deposit on anything interposed between the mouth and tube. The air breathed was inspired through the nose only while the whole of the air expired was driven through the tube, the nose being kept closed with the thumb and index. I found no difficulty in doing this with accuracy; great care was taken to keep the saliva from flowing into the tube together with the air expired. Except in the case of a few experiments at the summit of the Peak, I alone submitted myself to this part of the inquiry. The experiments were made by series of usually three at a time, the figures given in my table are the *means* of those of the different series.

The numbers actually obtained gave, of course, the weight of the moisture evaporated from the lungs, together with that of the atmospheric humidity of the air exhaled; a correction had, therefore, to be made. I determined the atmospheric humidity by means of dry and wet bulb thermometers, and the corresponding weight of moisture in a given bulk of air was taken from Glaisher's hygrometrical tables (fifth edition).

The results from these experiments have been condensed in the following table:—

Moisture Expired less Atmospheric Humidity Inhaled.

Stations.	Barometrical pressure.	Number of experiments.	Mean correction for humidity inhaled.*	Moisture expired corrected for atmospheric humidity.	Moisture (corrected) expired per litre.
	Inches.				
		Self.			
Summit of Peak, 12,200 feet.	17·993	3	0·043	0·324	0·0339
Alta Vista, 10,700 feet.	20·513	38	0·086	0·314	0·0330
Guajara, 7,090 feet	23·397	22	0·040	0·247	No determination.
Puerto, seaside ...	22·922	36	0·105	0·183	0·0237
		Cupelin.			
Summit of Peak, 12,200 feet.	17·993	3	0·060	0·459	0·0348

It was not without some trouble that a few successful determinations of the moisture expired were obtained at the highest point of the Peak, 12,200 feet above the sea. This summit is a cup-shaped depression, about half a mile in diameter, volcanic rocks towering round it. The depth of this crater does not appear to exceed 30 or 40 feet, and there is no difficulty in walking across it in any direction. The floor of the crater consists of a light white sandy material mixed at places with crystals of sulphur, while rocks crop out here and there. There was a great difficulty in finding a spot sheltered from the sun where I could place my balance and sit down to breathe through the tube. At last some shade was obtained for the balance by means of a blanket, and we managed to creep into a narrow place between two rocks, where the sun's rays could not penetrate. The heat was intense, the sun pouring down upon the Peak from a perfectly clear sky, and everything being nearly too hot to be touched, notwithstanding the intense terrestrial radiation at that altitude. Apparently every circumstance combined to baffle my experiments; the balance would not remain in a horizontal position; a light breeze kept blowing the fine sand about, and I had constantly to remove the beam of the balance to wipe the points of suspension; then the blanket would not keep in its required position; and I had to lay down at full length on the hot sand without any shelter from the sun to get through the weighings.

* Calculated from Glaisher's Hygrometrical Tables.

The few experiments I succeeded in completing at that spot, showed an evaporation of water from the lungs above that expired at the sea-side, equal to 0·141 grm. per minute, or 43·5 per cent.

If the weight of moisture expired at the three principal stations be considered together with the *altitudes of the stations*, a certain relation will be found to exist between them; this relation is established in the following table, showing what the proportions of humidity expired would amount to if calculated with reference to the barometrical pressures. These figures are entered in the column of the following table headed *Theory*.

Water Expired.

Barometer.	Theory.	Found.	Difference.
Puerto.... 760 millims.	0·183 grm.	0·183 grm.	
Guajara... 594·4 "	0·234 "	0·247 "	5 per cent.
Alta Vista. 521·4 "	0·267 "	0·314 "	15 "

The results obtained show, therefore, that the evaporation of moisture from the lungs increases as the barometer falls. The ratio is, however, no more than approximate. I question whether a similar result would be obtained in the Alps, where the cold at certain heights must exert a considerable influence on the evaporation from the lungs and air passages.

Results from the Investigation.

The results I have obtained from my experiments on the Island of Teneriffe may be expressed as follows:—

1. The mean of the whole amount of the carbonic acid expired at the three stations (the experiments at the foot of cone not included) in the sitting posture, and determined from 60 experiments in my case and 55 in that of the guide, was proportional to the weights of our bodies respectively, and amounted to 676 mgms. per 100 kilos. for each of us.

2. The mean weight of the whole carbonic acid expired at the three stations while engaged with the same amount of measured muscular work, and determined from 18 experiments for each of us, was respectively proportional to the weights of our bodies.

3. The mean weight of carbonic acid expired by both of us (with one exception only) was highest during the first or second hour after a meal, while it diminished by degrees as time elapsed since food was taken. This agrees with my results obtained in the Alps.

4. The mean weight of carbonic acid expired by myself on the

Island of Teneriffe is greater than it had been in the Alps, and, moreover, this same result holds good for corresponding altitudes. The mean excess for all the experiments on Teneriffe in the sitting posture, amounts for myself to 14·0 per cent. It was at the seaside that the increase in my case reached the maximum, 18·7, when compared with the weight of carbonic acid expired near the Lake of Geneva. I have only four experiments to place on record made on the guide in the Alps (St. Bernard); these compared with the means of the experiments to which he subjected himself at the seaside, Teneriffe, gave for the latter station an increased expiration of carbonic acid by 17·5 per cent. There was, however, no increase for the higher stations at Teneriffe.

5. While, in the Alps, the maximum quantity of carbonic acid was expired by myself at the highest station, 13,685 feet above the sea, where the body underwent the greatest degree of cooling, especially from the low temperature of the air; on the Peak of Teneriffe, the weight of carbonic acid I expired at the various stations differed but little.

6. The weight of carbonic acid expired in a given time by myself on the Peak of Teneriffe varies but little from one station to another, although I show a tendency to give out slightly more of this gas at the two highest stations—mean altitude 11,222 feet—than at either 7,090 feet high, or the seaside. The increase for the mean of the two highest stations above the amount expired at the seaside is only 1·2 per cent. In the Alps, the excess of carbonic acid I expired at 13,685 feet, over the amount given out near the Lake of Geneva at 1,230 feet, or for a difference of altitude of 12,455 feet, amounted to 15 per cent. This result is accounted for from the temperature of the air, which was much colder in the Alps than on the Peak of Teneriffe.

In the case of the guide, a great deal more carbonic acid was expired at the seaside on the Island of Teneriffe than on the Peak, the excess amounting to 17 per cent.; while I expired about as much carbonic acid at every altitude on that Island. This occurred apparently because the guide perspired more than I did at the higher stations; moreover, I am accustomed to live at the sea level, while the guide had never been away from the Alps, and his life, in summer, is spent, in a great measure, accompanying tourists to the highest peaks and passes in the Alps; his home at Chamounix is 3,451 feet above the sea.

7. The volume of air I expired per minute reduced to 32° F. and seaside pressure decreased gradually from the seaside to an altitude of 11,745 feet, the difference for the two extreme stations amounting to 14·6 per cent. This result agrees to some extent with that obtained in the Alps, although the Alpine decrease amounted only to 5·6 per

cent. The volume of air expired in the case of the guide exhibits a similar change, amounting to 22·6 per cent., but the decrease stops at Guajara, the intermediate station. The total mean volume of air expired per minute, at every station (the foot of the cone excepted), while in a sitting posture, was for myself 5·36 litres, and for the guide 6·75 litres.

8. The percentage of carbonic acid in the air expired exhibits nearly the same changes on the Island of Teneriffe as in the Alps. At Teneriffe it rose from 4·1 per cent. at the seaside to 4·9 per cent. at 11,945 feet, while, in the Alps, the proportion had varied from 3·8 per cent. at 1,230 feet to 4·7 per cent. (St. Bernard) at 9,403 feet. If the total mean proportion of carbonic acid in the air expired, reduced, for the three stations of Alta Vista, Guajara, and Puerto be calculated, it will be found to amount, for myself, to 4·4 per cent. and for the guide to 4·6 per cent., or to be nearly the same. The mean from the eighty-nine experiments I made in the Alps, in the sitting posture, yielded 4·2 per cent. of carbonic acid expired.

9. The frequency of the expirations fell considerably in both cases at the seaside, or increased on rising above the sea, but was much the same for each of us respectively at the different stations on the Peak. The reduction at the seaside, from the mean frequency of respiration at the upper stations, amounted for myself to 31·2 per cent., and for the guide to 25·5 per cent. In the Alps there had been a somewhat gradual rise of the frequency of the respirations between the lowest and highest stations, equal in my case to 34·9 per cent.

10. While raising with the feet a weight of 39·5 lbs. to an elevation of 5·06 inches forty-five times per minute, we both expired the least amount of carbonic acid at the lowest station, and the most at the intermediate station, 7,090 feet high. The fluctuation between the various stations was much the same for each of us respectively, although the actual amount expired by each of us differed in a marked degree. The mean relation for both of us respectively, between the carbonic acid expired sitting and on the rocking-boards, was found to be the same, and a trifle over twice the weight of the carbonic acid expired sitting.

The volume of air breathed while at work was decidedly less in my case at Alta Vista than at the two lower stations, with the guide there was a falling off in the air expired at Guajara. The mean volume of air expired per minute, in all the experiments on the rocking-boards, was for myself 11·56 litres, and for the guide 13·95 litres.

The general result obtained, with reference to this subject, was that the relation between the *volumes* of air expired while sitting, and while engaged with a regulated amount of muscular work, was the same as the relation found to exist between the *weights* of carbonic acid expired under such circumstances, and moreover that these pro-

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portions were practically the same for both of us. The relations are as follows:—

		Sitting.	Rocking-board.	Relations.
Self	Air expired...........	5·36 litres	11·56 litres	1 : 2·16
	Carbonic acid expired..	0·469 grm.	1·011 grms.	1 : 2·16
Cupelin	Air expired........	6·72 litres	13·95 litres	1 : 2·07
	Carbonic acid expired	0·603 grm.	1·269 grms.	1 : 2·10

As to the *frequency of the respiration*, while at work on the rocking-boards, it was the greatest with me at the highest station, and with the guide at the intermediate station; in both cases it was the lowest at the seaside. The mean frequency amounted, in my case, to 12·4 per minute against 10·2 sitting, giving a relation of 1·22; or for 1 respiration (expiration) sitting, I took 1·22 respiration on the tread-board. With the guide, the corresponding figures were 11·0 against 9·7, and the relation 1·13; so that for 1 respiration sitting, the guide took 1·13 respiration on the tread-board. His breathing while taking muscular exercise was, therefore, relatively rather slower than mine had been under similar circumstances.

11. The results obtained from the determination of *the water expired*, or evaporated from the lungs and air-passages, show distinctly that the moisture exhaled increases as a person rises above the sea. On the Island of Teneriffe, where the temperature in the shade is comparatively high, even at great altitudes, there is a tendency to the degree of evaporation being in an inverse ratio to the atmospheric pressure.

It is very obvious that this increased evaporation as altitude increased must have caused a corresponding loss of heat, or cooling of the lungs and air-passages; I felt this very much at night, when the temperature of the air frequently fell below freezing outside my tent. Of course, no number of blankets on our beds could check that source of cold.

The amount of water evaporated from my lungs and air-passages during twelve hours of daytime, calculated from the above data, would be—

At Alta Vista	226·1 grms.
At Guajara	177·8 „
At Puerto (seaside).. ..	131·7 „

The correction to be applied from the moisture present in the air breathed increased, of course, very much at the seaside, where it formed a considerable proportion of the moisture actually present in the air expired.

V. "Further Researches on the Physiology of Sugar in relation to the Blood." By F. W. PAVY, M.D., F.R.S. Received April 3, 1879.

The results brought forward in this communication are supplementary to those published in the "Proceedings of the Royal Society" for June, 1877 (vol. xxvi, pp. 314, 346).

The first of these communications was devoted to the consideration of the quantitative determination of sugar for physiological purposes. Some important physiological conclusions had been drawn by Bernard from the results obtained through a modified method introduced by him of employing Fehling's solution. I pointed out the manner in which I considered the process in question to be open to fallacy, and showed that the results yielded by it differed to a marked extent from those yielded by a gravimetric method, which I described, of using the copper test.

I have since continued my investigations, and have now results to bring forward obtained by another process, which I described in a communication read at the Royal Society, January 16, 1879, and published in the "Proceedings," vol. xxviii, p. 260. This process does not differ in principle of action from Bernard's, and if there were no fallacy in either case involved, the results yielded by the two should agree. In both the reduction of the oxide of copper is made to occur without the precipitation of the reduced oxide, so that the change to be watched in the action of the test is a progressive decoloration, unobscured by the presence of any deposit. In Bernard's process this result is brought about by the action of potash in a concentrated form upon the organic matter incidentally present in the product prepared for examination, and the agency in force is, according to the view I have expressed, the development of ammonia. In my own process, for particulars regarding which I must refer to the published communication in the "Proceedings," ammonia is added to the test, and no fixed alkali employed beyond that present in Fehling's solution.

By means of this new process an opportunity is afforded of ascertaining on which side the fault lies in the disagreement between the results obtained by Bernard's and the gravimetric method.

The accompanying table contains the results given by the application of the three processes to six specimens of blood.

The figures in the first two columns are derived from the analyses respectively conducted with the use of potash (Bernard's plan) and ammonia, and with the adoption of Bernard's proposition that the liquid obtained from equal weights of blood and sulphate of soda measures, in cub. centims., four-fifths of the total weight in grammes of the sulphate of soda and blood taken.

The figures in the next two columns represent the results given by the same two processes of analysis applied to the product obtained after the plan adopted for the gravimetric process. The blood is treated with sulphate of soda, filtered, the coagulum thoroughly washed to extract all the sugar, and the filtrate and washings brought to a known volume.

The last column furnishes the mean of two gravimetric analyses carried out upon two portions of the blood distinct from that employed for the analyses in columns 3 and 4.

Results given by Bernard's, the Ammoniated Cupric, and the Gravimetric Processes for the quantitative determination of Sugar in Blood.

Source.	Sugar per 1,000 parts.				
	With Bernard's formula for estimating volume of liquid derived from the blood.		Preparation of blood product by the process employed for the gravimetric method.		Gravimetric process. Mean of two analyses.
	Bernard's potash process.	Ammoniated cupric process.	Bernard's potash process.	Ammoniated cupric process.	
I. Sheep....	0·930	0·560	0·842	0·571	0·589
II. Bullock ..	1·212	0·901	0·980	0·650	0·735
III. Bullock ..	1·568	1·130	1·240	0·896	0·921
IV. Sheep....	0·888	0·579	0·905	0·567	0·533
V. Bullock ..	0·816	0·534	0·839	0·559	0·511
VI. Sheep....	0·879	0·635	0·945	0·650	0·631

On looking at the results the first point to which attention may be directed is that evidence is supplied showing that reliance cannot be placed upon the formula adopted by Bernard for calculating the volume of liquid derivable from the weight of blood taken for analysis. The figures in columns 3 and 4 were drawn from direct observation, and if the formula supplied correct information, the results in columns 1 and 3 and 2 and 4 should respectively coincide. It is noticeable, however, that whilst in some instances they approach closely towards agreement, in others there is a pretty wide divergence.

In the second place it is seen that the results obtained by the method of analysis involving the employment of the potash stand considerably higher than those yielded by the ammoniated form of the test. It may be assumed that in the action of the potash on the incidental organic matter present to give rise to the required condition for main-

taining the suboxide in the dissolved state, a reducing substance becomes developed, or else that the amount of oxide of copper appropriated to the oxidation of the sugar becomes altered in the presence of the large amount of potash which it is necessary to employ.

Lastly, it may be observed that a close conformity exists between the figures in columns 4 and 5. The results obtained by the gravimetric method are thus confirmed by the volumetric results obtained by means of the ammoniated form of the cupric test. Seeing that separately prepared specimens of the respective samples of blood were submitted to the two kinds of analysis, the conformity is certainly striking, and gives strong weight to the validity of the results yielded by the gravimetric method.

One of the points referred to in my second communication * is the spontaneous disappearance of sugar from the blood after withdrawal from the body. It is a part of Bernard's doctrine that the natural seat of destruction of sugar within the system is in the systemic capillaries, and if it can be shown that an active disappearance of sugar occurs in the blood after removal from the vessels, support is given to his proposition. I cited the observation which has been adduced by Bernard to illustrate that a marked aptitude exists for the disappearance of sugar under the circumstances named. According to this observation, a reduction from 1·070 to 0·880 parts per 1,000 occurred during the first half-hour, and at the end of 24 hours the analytical result obtained is represented as standing thus—0·000. I stated that my own experience furnished evidence of a widely different nature, and introduced the figures yielded by five observations in proof of this assertion.

The gravimetric process is only suited for the examination of blood before decomposition has set in, as the result would be vitiated by the presence of ammonia as a product of decomposition, the effect being an interference with the deposition of the cuprous oxide. With the ammoniated cupric test, however, any state is suitable; and since my communication of June 21st, 1877, was published, I have applied this process, as well as Bernard's potash process, to blood which has been kept for lengthened periods, instead of limiting the examination, as I had previously done, to the first twenty-four hours. The results obtained were quite unlooked for, and quite irreconcileable with the representation in Bernard's observation, which has been referred to, that at the end of twenty-four hours the blood ceased to give any indication of the presence of sugar.

I have before me a large amount of recorded experience, but need only select a few illustrative examples, for the information supplied is of the same nature throughout. In no case, although the blood had acquired a highly offensive character from putrefaction, has it failed to

* "Proc. Roy. Soc.," vol. xxvi, p. 346.

exercise a decided amount of reducing power over the copper test. It will be seen in the succeeding observations that there is a period during the first few days when the reducing power undergoes a pretty sudden fall, and that it afterwards remains nearly stationary. If the reducing action is to be attributed to sugar, and sugar only, it would have to be said that sugar can exist in a mass of putrefied blood without undergoing destruction. It seems to me, and this view is supported by evidence to be presently adduced derivable from the addition of sugar to decomposing blood, that there is another reducing substance present besides sugar which is not affected in a similar manner by contiguous decomposition. The period of sudden fall, it appears, may be taken as corresponding with the disappearance of sugar, and this, it will be noticed, presents a variation within certain limits. In the observations at the top of the list the period is more prolonged than in those lower down. It is possible that this may have arisen from the atmosphere of the laboratory having become influenced by the presence of decomposing samples of blood.

The earlier observations were conducted with the application of Bernard's process only, as it was not until June, 1878, that I began to apply the ammoniated cupric liquid. In the employment of this liquid the same product prepared from the blood was used as for Bernard's mode of testing. It is noticeable that the results yielded by the two processes differ from each other in the manner that has been already commented upon.

Decomposition of Blood in relation to Sugar.

	Reducing action, expressed as sugar per 1,000 parts.	
	Bernard's process.	Ammoniated cupric process.
I. *Blood of bullock.*		
Day of withdrawal	1·066	
3 days afterwards	0·987	
4 " "	0·808	
30 " "	0·215	
II. *Blood of sheep.*		
Day of withdrawal	0·909	
3 days afterwards	0·325	
4 " "	0·303	
8 " "	0·283	
21 " "	0·242	
30 " "	0·242	
III. *Blood of bullock.*		
Day of withdrawal	1·333	
1 day afterwards	0·365	
2 days "	0·353	
17 " "	0·259	
30 " "	0·242	

	Reducing action, expressed as sugar per 1,000 parts.	
	Bernard's process.	Ammoniated cupric process.
IV. *Blood of bullock.*		
Day of withdrawal	1·600	
1 day afterwards	1·311	
2 days ”	0·816	
4 ” ”	0·740	
7 ” ”	0·392	
17 ” ”	0·322	
26 ” ”	0·273	
V. *Blood of sheep.*		
Day of withdrawal	0·898	
1 day afterwards	0·851	
2 days ”	0·328	
4 ” ”	0·320	
7 ” ”	0·295	
17 ” ”	0·228	
26 ” ”	0·243	
VI. *Blood of sheep.*		
Day of withdrawal	0·930	
1 day afterwards	0·375	
17 days ”	0·180	
30 ” ”	0·181	
VII. *Blood of bullock.*		
Day of withdrawal	1·095	0·926
1 day afterwards	0·571	0·439
2 days ”	0·655	0·529
3 ” ”	0·351	0·317
4 ” ”	0·396	0·362
IX. *Blood of bullock.*		
Day of withdrawal	1·000	0·775
1 day afterwards	0·441	0·334
2 days ”	0·384	0·253
5 ” ”	0·310	0·231
X. *Blood of bullock.*		
Day of withdrawal	1·418	1·111
1 day afterwards	1·052	0·717
2 days ”	0·869	0·545
3 ” ”	0·369	0·294

In the following series of experiments sugar was added to blood at different periods after withdrawal. The results furnish the same kind of evidence as that obtained through the previous observations. A marked descent is noticeable in the reducing power until a certain point is attained, after which but little change occurs, however long the blood may be kept, and however putrid it may become. It seems, therefore, as already suggested, that there is a reducing agent in the blood which comports itself differently from sugar. If the reducing action were due solely to sugar, it is not intelligible that there should be a more or less sharp descent to a certain point, and then that

the condition should remain comparatively stationary. There is no reason that the last portion of sugar should behave differently from the first. Something having a reducing power, on the other hand, appears to exist which possesses a stability greater than that enjoyed by sugar, and which, thus resisting the influence of the changes of decomposition, produced the reducing effect on the test exerted by the blood after keeping for thirty days.

Observations on the Blood after the addition of Sugar.

	Reducing action, expressed as sugar per 1,000 parts.	
	Bernard's process.	Ammoniated cupric process.
I. Blood of sheep, in fresh state	0·919	0·723
After the addition of sugar	4·210	3·921
On the following day	1·481	1·419
On the 3rd day	0·409	0·267
On the 5th day	0·250	0·175
II. Blood of bullock, 4th day after withdrawal	0·351	0·317
After the addition of sugar	1·481	1·150
On the 3rd day	0·800	0·725
On the 4th day	0·333	0·293
On the 5th day	0·333	0·279
III. Blood of bullock, in fresh state	1·000	0·776
After the addition of sugar	4·444	3 636
On the following day	3·478	2·811
On the 3rd day	1·052	0·296
On the 6th day	0·363	0·228
The original blood to which no sugar had been added, examined on the 6th day	0·310	0·225
IV. Blood of bullock, in fresh state	1·000	0·776
After the addition of sugar	2·666	2·105
On the following day	2·285	1·960
On the 3rd day	1·176	0·980
On the 6th day	0·298	0·238
V. Blood of sheep, in putrid state	.. ..	0·234
After the addition of sugar	.. ..	0·834
On the following day	.. ..	0·476
On the 3rd day	.. ..	0·330
VI. Blood of sheep, in putrid state		
After the addition of sugar	.. ..	1·111
On the following day	.. ..	0·434
On the 3rd day	.. ..	0·300
On the 4th day	.. ..	0·256

	Reducing action, expressed as sugar per 1,000 parts.	
	Bernard's process.	Ammoniated cupric process.
VII. Blood of sheep, in putrid condition	.. ..	0·330
After the addition of sugar	.. ..	1·616
On the following day, the temperature having been meanwhile maintained at 90° F.	.. ..	0·250
On the 3rd day	.. ..	0·257
VIII. Blood of sheep, in putrid state..............	.. ..	0·225
After the addition of sugar	.. ..	1·960
On the 3rd day	.. ..	0·325

In a further series of experiments, blood with added sugar was subjected to the influence of a current of different gases. I was desirous of ascertaining if oxygen promoted the disappearance of sugar, and counterpart observations were made with carbonic acid and hydrogen. The results obtained afford no evidence of any chemical action being exerted. Whatever slight effect occurred, I think, may be assumed to have arisen from increased molecular action excited by the transit of the gas.

Passage of Gases through Blood in relation to the disappearance of Sugar.

	Sugar per 1,000 parts. By ammoniated cupric process.
I. Blood from sheep, in fresh state with sugar added	0·865
After standing 2 hours at the ordinary temperature	0·855
After the passage of oxygen for 2 hours at ordinary temperature..................	0·844
II. Blood from sheep, in fresh state, with sugar added	1·634
After standing 2½ hours at 100° F.........	1·459
After the passage of oxygen for 2½ hours at 100° F.	1·285
After the passage of carbonic acid for 2½ hours at 100° F.....................	1·100
III. Blood from sheep, in fresh state, with sugar added	1·667
After standing 7 hours at 100° F.	1·342
After the passage of oxygen for 7 hours at 100° F..............................	0·992
After the passage of carbonic acid for 7 hours at 100° F.......................	1·042

	Sugar per 1,000 parts. By ammoniated cupric process.
IV. Blood from sheep, in fresh state, with sugar added	1·850
After standing for 7 hours at slightly raised temperature	1·550
After passage of oxygen for 7 hours at slightly raised temperature	1·525
After passage of carbonic acid for 7 hours at slightly raised temperature	1·525
After passage of hydrogen for 7 hours at slightly raised temperature	1·570
V. Blood of sheep, in fresh state, with sugar added	1·475
After standing 6½ hours at 100° F.	1·324
After the passage of oxygen for 6½ hours at 100° F.	1·134
After the passage of carbonic acid for 6½ hours at 100° F.	1·209
VI. Blood from sheep, in fresh state, with sugar added	1·775
After standing 6½ hours at 100° F.	1·567
After the passage of oxygen for 6½ hours at 100° F.	1·475
After the passage of carbonic acid for 6½ hours at 100° F.	1·492
VII. Blood from sheep, in putrid state, with sugar added	1·324
After standing 6 hours at 100° F.	0·667
After the passage of oxygen for 6 hours at 100° F.	0·606
After the passage of carbonic acid for 6 hours at 100° F.	0·654
VIII. Blood from sheep, in putrid state, with sugar added	0·551
After standing 6 hours at 100° F.	0·218
After the passage of hydrogen for 6 hours at 100° F.	0·233

The following conclusions may be expressed as constituting the issue of the results recorded in this communication:—

That the results bearing on the physiology of sugar in relation to the blood derived from the application of the gravimetric process, and given in my former communication to the Royal Society (vol. xxvi, p. 346), are confirmed by those yielded by the ammoniated cupric test, described by me in the "Proceedings," vol. xxviii, p. 260.

That the disappearance of sugar from the blood after withdrawal from the system takes place in the gradual manner that might be expected from the effect of ordinary decomposition, and presents nothing to support any conclusion regarding the destruction of sugar in the blood as a physiological phenomenon.

That there appears to exist a reducing substance besides sugar in the blood which is of a sufficiently stable character to resist the effects of advanced putrefaction. Expressed as sugar, it amounts to from ·200 to ·300 per 1,000. These figures, therefore, would appear to require to be deducted from those which have been given as representing the amount of sugar present in the blood.

That the passage of a current of oxygen through the blood does not exert any appreciable effect in the direction of oxidation of the sugar.

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END OF THE TWENTY-EIGHTH VOLUME.

HARRISON AND SONS, PRINTERS IN ORDINARY TO HER MAJESTY, ST. MARTIN'S LANE.

OBITUARY NOTICES OF FELLOWS DECEASED.

The Rev. William Branwhite Clarke was born 2nd June, 1798, at East Bergholt, county of Suffolk, and educated partly in his father's house, under the Rev. R. G. Suckling Browne, B.D., a distinguished Hebrew scholar, and Fellow of Dulwich College, and partly at Dedham Grammar School; he entered into residence at Jesus College, Cambridge, in October, 1817. He took his degree in January, 1821, and in 1824 became M.A.

In May, 1821, he received deacon's orders from Dr. Bathurst, Bishop of Norwich, and priest's orders in May, 1823.

From May, 1821, to November, 1824, he was trained as curate at Ramsholt, at Nedging and Whatfield, at Chellesworth and Brantham, and then in his native parish. He took advantage of his rector's permission to travel every year, and thus laid the foundation of practical application of the geological and mineralogical lessons he had received at the University, under Professor Sedgwick and Dr. E. Clarke, the great traveller. He made a personal examination of the most celebrated formations of Europe. He travelled extensively in England and Wales and on the Continent from 1820 to 1839, not omitting a single year. He thus visited the Lake District, the Isle of Man, Staffordshire, Derbyshire, North Wales, the chalk and oolite of Yorkshire and Lincolnshire, the chalk of Sussex and Normandy, the central and southern parts of France, the Alps and North of Italy, the Netherlands, Rhenish provinces, Prussia, Belgium, the Ardennes, the tertiary districts of Nassau, the volcanic districts of the Rhine and Moselle. In 1829 he completed his survey of the counties of Suffolk, Norfolk, and Essex. In 1830 he visited the chalk districts and older formations of the frontiers of France and Belgium. Then followed Dorsetshire, West of England, Isle of Wight, Sussex, Southwest of England, the coal beds of the Boulonnais, the North of France, the Channel Islands and Isle of Portland, the new red sandstone of Staffordshire, Cheshire, and Lancashire, the Silurian old red sandstone and coal districts of Shropshire, Herefordshire, Monmouthshire, and South Wales.

In 1830 and 1831 he was present during many of the stirring scenes of the Belgian War of Independence and the last siege of Antwerp; at which time also he made the acquaintance of the lady who soon after became his wife, a daughter of Dr. Stather, a gentleman of

position in the Island of Nevis. This lady, with a son and two daughters, survives him.

In 1833 he was presented to a small vicarage near Poole, where in addition to his clerical duties he discharged the functions of a magistrate. His ardour in pursuit of travel and geology exposed him to a severe illness which culminated in rheumatic fever, which so crippled him that he was induced in 1839 to try the effects of a warmer climate, and as the investigation of a new country had peculiar charms, he resolved to visit the colony of New South Wales, which in those days included what are now Victoria and Queensland. He had also a kind of special mission from his brother geologists to investigate the carboniferous formation of Australia. The ship in which he was making the voyage touched at the Cape of Good Hope, and Mr. Clarke seized the opportunity of making a survey of and report upon the geology about Cape Town. From the time he landed in Australia, in 1859, to the day of his death, he never ceased pushing forward his researches into the unknown regions which lay before him. It is no exaggeration to state that he knew every inch of the greater part of New South Wales proper, and from constant investigation of reports by explorers and others he knew the general character and geography, almost topography, of Australasia.

The modest income which is supposed to be the lot of those who undertake the duties of a clergyman, and which in the case of Mr. Clarke, averaged, up to 1861, less than £200 per annum, inclusive of the grant of £1,000 made in recognition of his services, prevented his issuing well illustrated works. Most of his publications appeared in a very modest form, either as Parliamentary papers, newspaper letters, or as papers in the various scientific journals. Latterly, the Government Printing Office offered some relief from the expense of publication, and the last edition of his last work is creditable to that establishment. The Government always had a high appreciation of his services, and never failed when in difficulties to utilise his knowledge. Thus in 1851, when the Government wished to have a proper report upon the mineral resources of the country, no fitter person could be found. The neighbouring colonies also appealed to Mr. Clarke upon all matters of a geological nature. His name was in fact a "household word" all over Australasia.

It is proved beyond controversy that he ascertained the auriferous nature of the country in 1841, ten years before the popular date of 1851. The main conclusions at which Mr. Clarke arrived from his geological investigations were, that matrix gold was the thing to be looked for, and that the carboniferous deposits of the main seams in New South Wales were Palæozoic.

To a geologist of Europe, with libraries of reference in every city, and with rapid means of locomotion and comfortable quarters every-

where, the difficulties of Australian geology are not apparent. When Mr. Clarke set out on his explorations there was no other means of travel but horses, with pack-horses for provisions, tents, and instruments. In some parts of the country there were no roads or landmarks of any kind, and the maps of the district were nearly useless, as being only skeleton outlines of boundaries. He had to carry on his work single-handed, and gradually to form his own library.

Mr. Clarke's travels extended to Tasmania, Victoria, and Queensland, and he has written various exhaustive reports relating to these countries. His writings have guided persons to various profitable gold mines, and the successful tin industries of Australia and Tasmania have been commenced from indications furnished by him.

In 1863 the Legislature of New South Wales voted Mr. Clarke £3,000, at a time when his various ailments seemed coming to a head, to enable him to secure a little comfort in his old age; but since that time his "pen of a ready writer" has never been weary; and we are almost tempted to say that his latter years have surpassed the former, for his facts seemed to have accumulated more quickly, and his experience being, of course, more matured, enabled him to seize upon the more salient points of the geology of the country. The fruit of his labours during this part of his life consists of a geological map of the whole colony, which has been compiled from his note-books and memoranda. Up to 1870 he never ceased from the work of his sacred calling, even when on his explorations; but on 1st October of that year, his increasing infirmities obliged him to retire from his parochial labours. He was thus in a position to avail himself of the improved locomotion afforded by the railways to revisit his old haunts, and to visit other places of interest, so as to fill up gaps in his former works.

He was an indefatigable observer of meteorological facts and of general natural history.

Mr. Clarke was elected F.G.S. in 1826. He was a member of the Geological Society of France, and held a diploma from the Imperial and Royal Geological Institution of Austria, F.R.G.S., and one of the early Fellows of the Zoological Society.

Mr. Clarke contributed largely to the periodical literature of England prior to 1839, and his poetical effusions are by no means undeserving of praise.

In 1876 he was elected F.R.S., and in 1877 he was awarded the Murchison Medal of the Geological Society of London. The terms in which the award was made express the results of his geological labours in Australia.

The excessive heat of March, 1878, combined with the labour of preparing a new edition of "Sedimentary Formations of New South Wales," proved too much for Mr. Clarke's strength. He was seized with paralysis on the 16th of that month; and though he rallied, so

that he was able to move about without help, and to arrange and label fossils received from Professor de Koninck on the 15th June, he was seized with a violent pain in the heart on the morning of the 16th, and before medical aid could be procured he was called to his long-earned rest.

It is proposed by the Government to purchase his collection of minerals, fossils, library, and geological maps, and with them to form a nucleus, under the name of the "Clarke Collection," of a grand Mining Museum.

Adolphe Théodore Brongniart, son of the illustrious Alexandre Brongniart, and grandson of Théodore Brongniart, an eminent architect, was born at Paris on the 14th January, 1801. Educated for the medical profession, he soon gave up that career in order to devote himself, under his father's guidance, to scientific pursuits, and early took a place among the first living botanists. In his nineteenth year he published his first and only zoological paper, containing the description of *Limnadia*, a new genus of Crustacea. In the following year he established the genus *Ceratopteris* for a curious and anomalous aquatic fern. In his twenty-first year he published his first palæontological memoir on the classification and distribution of fossil plants. He reviewed, in this paper, the various plant-remains then known, and grouped them in 4 classes and 19 genera. This memoir has been described as the starting point of the intelligent study of fossil plants. From this beginning Brongniart continued his labours and expounded the fragmentary remains of extinct floras, and traced their relation to living plants, and their position in the vegetable kingdom. He was singularly fitted for this work, for he already had an extensive acquaintance with the structure and classification of living plants, and he had so digested his knowledge that he was able to utilize it in the study of these obscure fossils. Seldom has a man made a more brilliant *début* than Adolphe Brongniart. The memoir on the classification and distribution of fossil plants, and another, which threw an entirely new light on the subject of the fertilization of living plants, were already finished, if not published, when he was about twenty-four years of age.

The study of the reproduction of organic beings had, up to his time, made so little progress that Brongniart obtained little assistance from previous workers. Without committing himself to any of the theories which had been suggested in explanation of the processes of fertilization, he confined himself to the observation and arrangement of facts. "Il est certains sujets," he says, "dont la difficulté éloigne et rebute les observateurs, tandis que la grandeur de leurs conséquences excite au plus haut degré l'imagination des hommes disposés à se contenter d'une hypothèse. Quant à moi, j'ai cherché d'abord à les

oublier toutes, à réunir des faits bien observés, à déduire de leur comparaison des conclusions de détail, et à former du rapprochement de celles-ci une théorie propre à les représenter."

Brongniart showed how the embryo is formed, little by little, by a process which he did not hesitate to say is identical over the whole of the vegetable kingdom. Microscopically small plants, the most majestic trees of our forests, our cultivated plants, all are reproduced by one and the same process.

For some years Brongniart prosecuted his investigations of living plants, and published, between 1824 and 1827, a new classification of fungi, a memoir on the natural order Bruniaceæ, and several histological and physiological memoirs. The labours and observations of six years in Palæontology were exhibited in his "Prodrome d'une Histoire des Végétaux Fossiles," puplished in 1828, and in his great work, "Histoire des Végétaux Fossiles," begun in the same year. The first volume, consisting of twelve parts, and containing 488 pages of letter-press and 160 quarto plates, was issued within a few months. The further progress of the work was interrupted by Brongniart's ill health, and it was not resumed for nine years, and then only three additional parts were issued, leaving this great work incomplete. The "Histoire" includes the whole of the Cryptogams, vascular as well as cellular, with the exception of the Lycopodiaceæ, to which the later plates are devoted; and the letter-press is suddenly stopped before the remarkable introduction to this natural order was completed.

In 1839 he brought out his well-known memoir on *Sigillaria elegans*. In 1849 he contributed to the "Dictionnaire Universel d'Histoire Naturelle," a short and popular review of fossil plants on the plan of his "Prodrome," exhibiting their botanical affinities and classification and their stratigraphical distribution.

It is worthy of notice that, as Brongniart began his scientific career with his labours on fossil plants, so the latter part of his life was devoted to a further investigation on the same subject. At the time of his first investigation only few and imperfect specimens had been obtained. Fifty years later his friends, Messieurs Renault and Grand d'Eury, sent him, from the environs of Autun and St. Etienne, a quantity of specimens of seeds which had been converted into siliceous masses of a texture as fine as that of the most beautiful agates. From these masses Brongniart separated transparent flakes, and, by the aid of the microscope, made out minute details of their organization, cells with excessively thin walls, vessels with delicate membranes, nebulosities which are the first indications of the formation of tissues, organisms, in fact, of dimensions so small as can only be detected by high powers of the microscope. He found, in fruits dating from remote ages, all the particulars of organization which he had formerly observed in living plants. No one had hitherto imagined it possible

that we should one day see in the substance of a hard and translucent stone indications of the sap which had, in former ages, circulated in the delicate vessels of a living plant, of the grains of pollen bursting from the anthers, and of the presence of the minute ovules where appearing. In his memoir on this subject, the last he ever published, Brongniart not only disclosed the evidence of a remarkable and varied gymnospermous vegetation in Carboniferous times, but the structure preserved in these Palæozoic plants led him to suspect the existence of a curious and hitherto unobserved detail in the organization of the ovule of living Gymnosperms. After having satisfactorily shown that the beautifully silicified tissues of the plants of St. Etienne belonged to plants of which we find analogous species in Mexico, he confidently asserted that a peculiarity, a cavity for the reception of the pollen, never previously observed in living specimens, would be found in the species of that country, and he subsequently had the satisfaction of exhibiting, to the Académie Française, some plants living in the hothouses of the Museum which had a pollen cavity of which a plant dead countless ages ago had furnished us with the first example.

Brongniart did not live to complete this important research. The decline of his health first manifested itself in a failure of sight brought on by excessive use of the microscope, and he owed it to the assistance of his friends and colleagues, Messieurs Bureau, Cornu, Renault, Grand d'Eury, and other friends, that he was able to work on at the investigation as long as he did. From the time of the siege of Paris his health, affected by the privations and sufferings he then underwent, steadily declined, but he retained to the end of his life his tranquillity of mind, his intellect, and his memory. He took an affectionate interest in the progress of a grandson, whose first steps in the career of science he hoped to guide. At length, in February, 1876, foreseeing that his end was near, he desired to be surrounded by his family, and expired in the arms of his eldest son.

Less fortunate than his father, Adolphe Brongniart had, some years previously, lost the affectionate wife whom he had married in early life. He left two sons and several grand-children.

The herbarium left by A. Brongniart has been placed in the Botanical Gallery (of Paris), and his unique and beautiful collection of fossil plants forms one of its greatest ornaments.

Brongniart was elected a member of the Academy of Sciences in 1834, in the place of Desfontaines, and in the same year he was appointed Professor of Vegetable Physiology in the Museum of Natural History. In 1840 he was appointed a foreign member of the Geological Society of London, and in the following year he received the Wollaston Medal, in consideration of his important works on fossil plants. In 1852 he was elected a foreign member of the

Royal Society, and was also appointed Inspector-General for Science of the University of Paris.

With regard to his scientific character, Brongniart has been characterized as the Linnæus of fossil botany; not so much a great discoverer as a great systematizer; introducing lucid order and general principles into the study of the materials which had been already collected. To those materials, also, he undoubtedly added much by his own observations, and probably (as in the case of Linnæus) his example gave a stimulus to the exertions of his opponents as well as of his followers. He was eminent, not only for industry, accuracy, and judgment, but also for the clearness and neatness of his scientific writings.

Brongniart's favourite branch of study is one in which exceedingly rapid progress has been made, since he was at the height of his fame, and in which rapid progress is still making. The researches of Heer, Unger, Ettingshausen, and others, in the fossil plants of the Tertiaries, have opened to us almost entirely new departments of Palæo-botany; the microscopic studies, which have been followed up with so much zeal and success by some in our own country, have thrown a greater amount of new light on the structure of the Palæozoic vegetation. But the name of Adolphe Brongniart deserves to be held in honour as long as the sciences of botany and geology are cultivated; and, however far the knowledge of these subjects may be carried, such works as his treatise on the structure of *Sigillaria* must always be valued as models of accurate examination, lucid exposition, and caution in drawing conclusions.

Adolphe Brongniart's careful investigation and illustration of the veining of recent ferns (*see* his "Histoire des Végétaux Fossiles," vol. i, p. 148) probably suggested some of the more recent methods of arranging that family of plants.

As a teacher he was remarkable for courtesy and kindness, and readiness to help students in that branch of science to which he had devoted himself.

Elias Magnus Fries was born at the parsonage of Femsjo, in Småland, in the southern part of Sweden, on the 15th August, 1794. He appears to have inherited from his father a love of natural history, and his parents carefully fostered and encouraged this taste, in hopes of thereby supplying to him the place of companions and playmates. At the age of twelve he was already familiar with many of the plants of the neighbourhood. In one of his rambles, in 1806, his attention was attracted by the large and peculiar *Hydnum coralloides*, and it was this discovery, he said in after years, which awakened in him a desire to study the Fungi. The very next day he set to work and learnt the few genera then known from Liljeblad's Swedish Flora. In the year 1808, when Sweden was ravaged by war and disease, it

became necessary to close the school at Wexio, which Fries attended, and he remained for a time at home. He made use of this period of leisure to describe all the Fungi he could find, and before 1811 he had succeeded in distinguishing three or four hundred species, but not having access to books on the subject, he gave them temporary names.

In 1811 he left the Gymnasium, and went to the University of Lund. At Lund he continued to give all his spare time to Botany, and had the satisfaction of finding many plants new to him. He spent much of his time in the library studying botanical works, in which he found the names of many of the species he had described. While at Lund he was fortunate enough to make the acquaintance of two distinguished naturalists, Retzius and Agardh, who put into his hands the mycological works of Persoon and Albertini, the best then existing. During the year 1812 he studied Hypodermia (Ustilagineæ, Æcidomycetes). While earnestly studying for the degree of Doctor of Philosophy he still found some leisure for his favourite pursuit. He took the degree of Ph.D. in 1814, and was also in this year appointed Docens of Botany. In 1819 he became "Adjunkt" and received the title of Professor in 1834.

From the time of taking his degree Fries devoted himself to the study of the Fungi, and went with this view for a time to Copenhagen. About the year 1814 he brought out his earliest important work, his "Novitiæ Floræ Sueciæ;" and his "Observationes Mycologicæ" was published, in the years 1815—1818, at Copenhagen. In 1814 he began to write his "Monographia Pyrenomycetum Suecia," which work he presented in parts from 1816 to 1819 to the Academy of Sciences, in Stockholm.

In the year 1816, having come to the conclusion that the method of describing and classifying hitherto adopted was by no means satisfactory, Fries began to work out a new system and to make fresh investigations of all the Fungi. This new system was based upon a minute examination of their different stages of development, and of the morphological relations of their different parts. The result of this investigation—the "Systema Mycologicum," in three volumes—he published between the years 1821 and 1829, and a supplement appeared in 1830.

In 1828 Fries published his "Elenchus Fungorum," in which he described some of the Fungi, of which great quantities had been sent to him from abroad.

Hitherto Fries had been absolutely prevented by want of means from indulging his ardent wish to explore foreign countries in search of specimens, but in the year 1828 he was at length able to visit the northern part of Germany and the Museum at Berlin, and had the opportunity of extending his knowledge of Lichens, of exotic Fungi, as well as of studying the literature of these plants.

After the publication, in 1829, of the third volume of his "Systema Mycologicum," he again subjected the Fungi to a close investigation, comparing them with his own descriptions. Having thus revised and completed his observations, separated the Discomycetes from the Hymenomycetes, &c., he published the results of his observations in his "Flora Scanica" in 1835.

Fries became Demonstrator in Botany at the University of Lund in 1828. In 1834 he was translated to the University of Upsala as Professor of Rural Economy, with which, after the death of Professor Wahlenberg in 1851, the chair of Botany was united. He discharged these teaching duties until 1859, when he retired on a pension.

At Upsala he found new fields for his mycological studies, and published his "Epicrisis Systematis Mycologici seu Synopsis Hymenomycetum" in 1836—1838.

In the year 1844 the Academy of Science in Stockholm resolved to be at the expense of a series of engravings of all the species of Fungi principally belonging to Hymenomycetes that could not be preserved in a natural state, and gave the superintendence and direction of this work to Fries. This collection, containing now from 1,600 to 1,700 coloured figures, is one of the richest and most extensive in existence. Eleven parts, with 110 plates, have been published under the title "Icones selectæ Hymenomycetum nondum delineatorum." These admirable figures, to the preparation of which his latter days were devoted, afford great help to the student in one of the most difficult parts of botany.

The last large work of Fries was the "Hymenomycetes Europæi sive Epicriseos Systematis Mycologici, editio altera," published in Upsala, in 1874.

Fries had also, at an early age, studied the Lichens no less thoroughly than the Fungi, and he essentially reformed the descriptions and systematic arrangement of these plants. His "Lichenographia Europæa reformata," published in Lund in 1831, was long regarded as a principal work in lichenographical literature, and the successively published parts of his "Lichenes exsiccati Sueciæ," form a remarkably valuable series.

He also published explanations and critical examinations of some of the more difficult genera among the higher plants, for instance *Hieracium*, *Salix*, *Carex*, and several others. He wrote Floras of the whole of Scandinavia, and of separate parts of it, and in his "Novitiæ Floræ Sueciæ," "Botaniska Notiser," &c., he gave descriptions of many new plants discovered by himself.

His Herbarium Normale, collected at great expense, and with incredible industry, contains dried specimens of many of the rarest plants of Scandinavia. It was issued in fifteen numbers, during a period of over twenty years, the last being dated 1857, and is con-

sidered to be of the greatest value for the study of the plants of northern Europe. This collection of typical plants is quoted by Fries throughout the first part of his "Summa Vegetabilium Scandinaviæ."

Fries has also written treatises on Agriculture and on Practical Botany, on the Nomenclature of Plants, and on the History of Botany. In the "Botanical Excursions," 1852—1864, he has very successfully popularised his science, and the book has been read with lively interest beyond his own country.

Important, however, as were many of Fries' labours on Phænogamic plants and Lichens, his future fame must rest upon his reformation of Fungology. The brilliant discoveries of later observers seem at first sight to eclipse altogether what was done so many years before, but they are quite in a different line, and doubtless have been assisted by the labours of Fries. His arrangement of the genus *Agaricus* alone has been described as a great effort of genius, and every division of his mycological system is full of matter for reflection. To appreciate his system, full allowance must be made for the state in which he found mycology and the comparative imperfection of microscopes.

Fries was eminent as a systematic botanist, and the Friesian system is still followed by some Swedish writers. The system was first published in the "Flora Scanica," 1835), and an outline of it will be found in Lindley's "Vegetable Kingdom."

With regard to the relationship of species, his point of view appears to have been the same as that taken by Linnæus, "A species is each form brought forth by the Creator in the beginning."

Fries had remarkable fluency and power of expression both in writing and lecturing, and this faculty no doubt contributed much to his influence in gathering round him a large number of disciples. Foreign scientific men seldom visited Upsala during the last forty years of his life without making the acquaintance of the celebrated botanist, whose amiable and engaging manners and kind disposition made him beloved by all who knew him.

Fries continued his scientific labours into the last years of his life. In his eightieth year he published a new and improved edition of his extensive work "Hymenomycetes Europæi," and about a week before his death he completed an essay for a foreign periodical. He died on the evening of the 8th of February, 1878, to the last actively useful.

In 1851, Fries had been appointed Director of the Botanical Museum and garden attached to the University of Upsala, and in 1853 became Rector of the University. He was a member of many learned societies, Swedish and foreign. In 1835 he was elected a Foreign Member of the Linnæan Society, and in 1875, a Foreign Member of the Royal Society.

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